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Wittgenstein On The Philosophy Of Mathematics

Raymond Haig Melkom

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> For example, does it not require some pains and skill to form the general idea of a triangle (which is yet none of the most abstract, comprehensive, and difficult), for it must be neither oblique nor rectangle, neither equilateral, equicrural, nor scalenon; but all and none of these at once.[1]

Since the abstract triangle represents every triangle, anything which is known or proved of it is also known or proved of every particular triangle.[2]

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WITTGENSTEIN ON THE PHILOSOPHY OF MATHEMATICS

by
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ABSTRACT

My main area of concern is Wittgenstein's philosophy of mathematics, primarily as it is expressed in his Remarks On The Foundations of Mathematics (henceforth abbreviated as Remarks). There is considerable controversy regarding the extent to which Wittgenstein understood the mathematical content of the views which he criticizes; among my primary aims will be an attempt to evaluate the degree to which he is faithful to the main theorems with which he deals. A number of approaches to the philosophy of mathematics will emerge in this examination of Wittgenstein's remarks, and I will be concerned to argue that these approaches can be effectively unified within the Wittgensteinian conception of a language-game; indeed, I shall argue that mathematics is most properly viewed as a collection of language-games, related to each other along the now familiar lines of "family resemblance".

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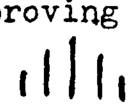
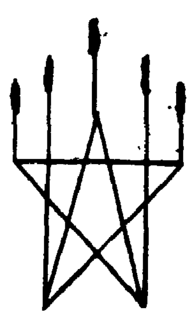
CHAPTER ONE

THE INEXORABILITY OF MATHEMATICS

(A) Rules And Mathematics

There is a certainty and rigor in mathematics which makes it a very fruitful field for investigation. "'But then what does the peculiar inexorability of mathematics consist in?' - Would not the inexorability with which two follows one and three two be a good example?" (<u>Remarks</u> p.3,no.4) In his efforts to discover the roots of this mathematical strictness, Wittgenstein resorts to an examination of some simple examples of reasoning. As we discover why these examples are convincing we will be in a better position to comprehend the nature of mathematical reasoning.

We will first turn our attention to the methods employed in proving that there are as many lines in the hand pattern  (H) as there are angles in the pentacle  (P). This fact may be established by correlating H and P as follows:



(<u>Remarks</u> p.10,no.25)

A mathematical proof must possess the characteristic of being permanent. The equation "7+5=12", for example, is always true. Any satisfactory account of mathematics must deal with this aspect of permanence. In the case of the hand pattern H and the pentacle P Wittgenstein's task is to present an analysis of the situation which firmly secures the identity of the number of lines of H with the number of angles of P.

One view which Wittgenstein rejects is the attitude that correlating H and P in the aforementioned manner demonstrates that "...it is of the essence of H and P to be the same in number" (Remarks p.12,no.31). He argues that the correlation in itself simply demonstrates that this particular H is related to this particular P by means of the given correlation; we are not given that any H and P bear this relation. There is a possibility that a person may not arrive at the same conclusion. Certainly, as the figures become more complicated, the likelihood of reaching different conclusions will increase. If someone does go wrong in implementing such a procedure we are not swayed by his peculiar result. We know that a mistake must have been committed.

Our assurance is based upon the fact that when we are faced with an H and P pattern we do not have to draw the correlating lines anew; rather we state that the particular H and P are of the same number on the basis of the fact that this H is an H and this particular P is a P and then employ the original proof as our justification. The proof serves as a prototype for understanding that the number of lines in H is the same as the number of angles in P. The essence comes after the prototype.

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Wittgenstein's remarks are part of an old problem in philosophy and are best seen in this perspective. This problem involves the universality within mathematics. How can a proof which deals with a particular have universal applicability?

Locke thought that it is by way of abstraction that a proof achieves universality. The abstract idea of a triangle, according to Locke, is ambivalent with respect to any property of a triangle.

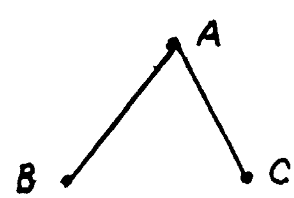
> For example, does it not require some pains and skill to form the general idea of a triangle (which is yet none of the most abstract, comprehensive, and difficult), for it must be neither oblique nor rectangle, neither equilateral, equicrural, nor scalenon; but all and none of these at once.[1]

Since the abstract triangle represents every triangle, anything which is known or proved of it is also known or proved of every particular triangle.[2]

This is an unsatisfactory explanation inasmuch as it is built upon a mistaken notion of what actually constitutes an abstract idea. Berkeley's criticism of Locke's notion brings to light the fact that it is impossible to form an abstract idea of a triangle which matches Locke's criterion of not being oblique, rectangle, equilateral, equicrural or scalenon while at the same time being all of these.[3]

Berkeley proposed an alternative to Locke's explanation. We can prove that all triangles have a certain property if we prove that a particular triangle has that property without, in the course of the proof, mentioning any of the properties that are peculiar to this individual triangle.[4]

This position is not as clear as one would like it to be. Berkeley does not set forth all of the reasons which might give credibility to his theory. Geometrical constructions in many cases depend, but not in a critical way, upon a particular characteristic of a figure. An example of this is found in Euclid's bisection of a rectilineal angle.[5] Let BAC be such an angle.



The first step is to take a random point D on BA. We may quite properly take D as that point on BA which is .75" from A. This depends upon the fact that BA is at least .75" in length. The universality of the proof is not in the least impaired by the accidental fact that BA happened to be at least .75" in length; however, Berkeley's description of what constitutes a proof may easily be interpreted to exclude this construction.

Kant's treatment of this problem is very interesting. It has an important place in the philosophy of mathematics and cannot be overestimated. The essentials are included in the following citation:

> ...mathematical knowledge is the knowledge gained by reason from the construction of concepts. To construct a concept means to exhibit a priori the intuition which corresponds to the concept. For the construction of a concept we therefore need a non-empirical intuition. The latter must, as intuition, be a single object, and yet none the less, as the construction of a concept (a universal representation), it must in its representation express universal validity for all possible intuitions which fall under the same concept. Thus I construct a triangle by representing the object which corresponds to this concept either by imagination alone, in pure intuition, or in accordance therewith also on paper, in empirical intuition - in both cases completely a priori, without having borrowed the pattern from any experience.[6]

A concept in mathematics gains its worth, so to speak, through the exhibition of a particular object which is subsumed under the concept. Kant considers it possible for a drawn triangle or the figure BAC, considered earlier, to support necessarily and universally their respective concepts. Kant achieves this goal by a clever combination of the method of construction with his doctrine of schemata.

> Accordingly, just as this single object is determined by certain universal conditions of construction, so the object of the concept, to which the single object corresponds merely as its schema, must likewise be thought as universally determined.[7]

Actually, it is not the single object that is the schema. The schema of the concept will consist of the universal conditions of construction that resulted in the single object. Thus, a drawn triangle cannot be the schema for the concept of a triangle whereas the universal rule according to which the particular triangle was drawn will provide a schema for the concept of a triangle. This interpretation is consistent with Kant's original formulation of his schematic doctrine wherein he declares that,

> No image could ever be adequate to the concept of a triangle in general...The schema of the triangle can exist nowhere but in thought. It is a rule of synthesis of the imagination, in respect to pure figures in space.[8]

A geometrical construction applied to a particular triangle in a proof will enable us to attribute the proven property to all triangles on the ground that the rule of construction may be applied to all triangles. This is an improvement on Berkeley's theory since it removes any dependence upon some accidental characteristic of the construction. We cannot apply our criticism of Berkeley to Kant for, despite the fact that our particular construction employed the incidental feature that BA's length was at least .75", the rule according to which the construction was performed, Euclid's procedure in Bk. I, Prop.9, is totally independent of the particular length of BA. This illustrates the salient point of Kant's approach - he separated the rule from the construction that it governed. While the latter is inherently not universal, the former is undeniably so.

We may now return to Wittgenstein's H and P with the intention of comparing his theory of proof with those of the philosophers considered above. Wittgenstein does not have any substantial agreement with Locke's conception of an abstract idea of a triangle. Although the original correlation between H and P is taken as a prototype, this does not commit Wittgenstein to the belief that the first H and P possess in some sense every property which any H and P may happen to have. First of all, Wittgenstein's prototype is not an abstract idea. It is a concrete drawing. Furthermore, the figures H and P represent other H's and P's by virtue of the names given to these figures.

> Now what was the point of our proposal to attach names to the five parallel strokes and the five-pointed star? What is done by their having got names? It will be a means of indicating something about the kind of use these figures have. Namely - that we recognize them as such-and-such at a glance. To do so, we don't count their strokes or angles; for us they are typical shapes, like knife and fork, like letters and numerals (<u>Remarks</u> p.15f,no.41).

At the moment when we recognize two patterns as being an H and a P, it is not necessary for the two patterns to be exactly the same as the original H and P for us to know that they are of the same number. The reason for this, is that the original proof impressed a procedure upon us (Remarks p.15,no.40). That is to say, the method of drawing the lines of correlation is impressed upon us. Hence, Wittgenstein understands the universality of the original proof to reside in the procedure of correlation which it employed. The relationship between a proof procedure and the following of a rule is very strong in Wittgenstein:

> The proof (the pattern of the proof) shows us the result of a procedure (the construction); and we are convinced that a procedure regulated in this way always leads to this configuration (Remarks p.75,no.22); Proof must be a procedure of which I say: Yes, this is how it has to be; this must come out if I proceed according to this rule (Remarks p.75,no.23).

Kant and Wittgenstein are similar insofar as they both account for the universality of a mathematical proof in terms of the rule of construction. One of the major differences between these two occurs in how they look upon rules and mental faculties. Kant does not give a great deal of information concerning his theory of rules; however, he does mention a few things about rules which provide a definite contrast to Wittgenstein's thoughts on the subject.

Kant's point of emphasis is that mathematical rules arise from a mental faculty, namely, the understanding[9] and are applied to the sensibility. Since these universal rules govern every step in mathematics,[10] mathematics is intimately connected with the faculty of understanding. In fairness to Kant, I should like to note that he applies these rules to concrete mathematical entities such as drawings and symbols.[11] The importance of notation will be dealt with later; our present task will be restricted to the rules governing the notation.

There is no necessary connection between rules and mental faculties in the thought of Wittgenstein. Normally one speaks of inferring as an activity which takes a person from one statement to a second one that is derived from it. In many cases where an inference is performed according to a rule, there is no obvious action occurring between the two statements. The temptation is to assume that the activity must be a mental activity.

> Misled by the special use of the verb "infer" we readily imagine that inferring is a peculiar activity, a process in the medium of the understanding, as it were a brewing of the vapour out of which the deduction arises (Remarks p.5,no.6).

To infer according to a rule, like any thoughtful use of language, does not presuppose a mental process which must accompany the statements of the language.[12]

There are times when inferences according to a rule take place in the head, as it were; but these are not given special status. It is for this reason that Wittgenstein dismisses any mystery from the process of derivation by means of a rule.

> There is nothing occult about this process; it is a derivation of one sentence from another according to a rule; a comparison of both with some paradigm or other, which represents the schema of the transition; or something of the kind. This may go on on paper, orally, or 'in the head' (Remarks p.5,no.6).

(B) Following A Rule

The aspect of Wittgenstein's philosophy of mathematics which distinctly sets it apart from other philosophies of mathematics is the notion of following a rule. Thinkers such as Kant, Frege and Hilbert devoted themselves to the task of clarifying the nature of mathematical rules without considering how one in fact follows a rule. Wittgenstein, on the other hand, spent his effort into the investigation of what it means to follow a rule. He certainly did not assume that there was anything inherent in the rule itself which would specify exactly how the rule was to be followed. The issue was more complicated than this for Wittgenstein. We will now examine those considerations which Wittgenstein believed to be involved in the notion of following a rule.

Let us consider, with Wittgenstein, the example of the student who continues the series of +2 beyond 1000 as 1000, 1004, 1008, 1012.[13] When the teacher informs the student that there are errors in his continuation of the series, the student may reply that he continued the series in the same way as he was taught to continue the series +2 up to 1000. Wittgenstein correctly points out the fact that in this case it would not help for the teacher to repeat the examples and explanations by which the student was taught, as the student has taken these examples and explanations as the method which yields the sequence 1000,1004, 1008, 1012.

The next point made by Wittgenstein is that there are major difficulties in clarifying a statement such as "The right step is the one that accords with the order - as it was meant".[14] This last statement attempts to give a procedure by which one will know how to continue the series at each step. The person who makes this statement cannot truthfully say that at the time of the order to write the series +2 he was thinking of each individual step of the series. There are an infinite number of steps involved in the series, thereby preventing one from being able to turn one's attention to every particular step. That is to say that the order "+2" is given without the instructor having the step 7386, 7388, for example, explicitly before him. Thus, whatever one means in giving an order such as "+2" should not lead to phrases of the following kind: "The steps are really already taken, even before I take them in writing or orally or in thought."[15]

Furthermore, any attempt on the part of the instructor to clarify his order to the pupil will involve words such as "same", "accord", "rule", "understand", etc. These attempts at clarification could be met by the student's response that the teacher is misapplying these words (<u>Remarks</u> p.33f.,nos.113,115). All of this will lead to the paradox that for any rule there is no particular action that is determined by the rule since every action can be taken to be in accordance with the rule.

Wittgenstein totally rejected this conclusion since it is based on the mistaken belief that a person interprets a rule whenever he follows a rule.[16] In the case which we have just examined the teacher might have justified the writing of 1000, 1002, 1004 by reference to the fact that it was in accordance with the rule. He may have subsequently argued that it was in accordance with the rule "+2" on the basis of the fact that the sequence 1000, 1002, 1004 was constructed by the same procedure that constructs the sequence 0, 2, 4. The teacher is thus providing the student with a series of interpretations. "Following the rule "+2"" is interpreted in terms of "accord" which in turn is interpreted by means of "same". Within this state of affairs we could never be certain that a rule was in fact being correctly followed for such terms as "agreement" and "same" already presuppose the following of a rule for their correct application.[17]

We would be left with a chain of interpretations, each of which would require a subsequent interpretation for support.

Wittgenstein concluded from this that there is a way of following a rule which is not an interpretation. This way of following a rule is made manifest in those particular instances which we describe as following the rule or not following the rule.[18] These particular instances can be seen in how one is taught to follow a rule. A number of examples are given to the student which illustrate what in fact is or is not in accordance with the rule. A good teacher will anticipate possible misunderstandings and demonstrate why these misunderstandings of the rule disobey the rule. The teacher will give signs of acceptance or disapproval as they are appropriate. The teacher will watch the student's procedure in carrying out the rule to be certain that he does understand how to follow the rule and does not have to get the answer from a book or other student. It is this type of process that teaches one how to follow a rule. After a period of time the teacher will have no further examples or explanations which will make the student's understanding of the rule any clearer. At this point the teacher has exhausted his discursive knowledge of the rule.[19]

If the student is still unable to follow the rule it is not on account of some additional piece of information that the teacher should have passed on to him. The student is lacking not in knowledge, but in ability. The student may have the same discursive knowledge about the rule as the teacher, yet the teacher can follow the rule while the student cannot. The difference between the student and teacher comes out when the student makes mistakes in particular instances of following a rule. The student does not act in the manner that is appropriate to the rule when he writes "1000, 1004, 1008, 1012". Despite the fact that the student may be able to recite all of the teacher's illustrations and explanations, we must say that this student does not understand the rule. He does not understand the rule because he is not able to continue the series.

The actions which meet the requirements of following a rule constitute a practice. The practice which supports the following of a rule is fundamental. Our language could not function without this practice.

The point which Wittgenstein makes here is important for mathematics.

> Disputes do not break out (among mathematicians, say) over the question whether a rule has been obeyed or not. People don't come to blows over it, for example. That is part of the framework on which the working of our language is based (for example, in giving descriptions).[20]

Mathematicians would not be able to calculate together or understand each other's results if they did not share in the practice of following a rule. The same situation would be present here as occurred in the example of the student who could not follow the rule "+2". Two mathematicians might agree on a set of equations and rules for the manipulation of equations and yet disagree as to the particular equations which result from the initial equations and rules. One of the mathematicians would say that an equation E results from equation A and rule R while the other mathematician would say that the first mathematician was wrong and that equation F results from equation A and rule R, where E and F are different equations.

Under these circumstances, where there is no practice of following a rule, there would be no use for mathematical language as there would be no objective criteria for the claim that a certain mathematical equation followed from another by the application of a rule. The disagreement between mathematicians would be so severe that there would be no mathematical information communicated when one made a statement of the type which we have been considering to another.

Let us now look at the other example which Wittgenstein gave in the preceding quotation, namely that following a rule is part of the framework which makes it possible to give descriptions. We will examine the manner in which the word "red" is used to describe things.

A person can tell someone that "My car is red" or "The room you are looking for has a red door". People can understand these statements and use them to communicate only if they share the common practice of following a rule. If person B told person A that A's car was not red but blue and if this should occur under normal circumstances where there is sufficient light and they both look at the same car which is painted in a primary colour, then there is going to be a conflict. Colour samples might be obtained from a paint store. They might agree that a particular sample is red but still disagree as to the colour of A's car. A would likely ask B if he could not see that the car and the sample have the same colour. B's reply would be that they are not the same. They would not be able to resolve their conflict. If this should happen often enough among us, it would be safe to assume that colour words such as "red" would not be able to function in our language.

The fact that we use colour words in our language and can make ourselves understood indicates that there is agreement as to the cases in which two objects have the same colour. This follows from the fact that we have the ability to follow such rules as "identify the red colour sample" or "indicate which two samples have the same colour". We do not behave in the same manner as the student who could not follow the order "+2". After being given a number of instructive examples we are able to properly use the word "red" in describing new objects.

(C) Following A Rule And Conventionalism

I would now like to establish the fact that the position which Wittgenstein has adopted in regard to following a rule is not the position of a conventionalist. I intend to achieve this by looking at the simple equation "2+1=3" and the reasons for accepting it as true.

Why is "2+1=3" true? If this equation were true simply because of its being a part of a game such as checkers we would have good reason for looking upon it as an arbitrary equation. One could play other games where "2+1=4" would be true. In this case arithmetical truths would depend on the type of game which we choose to play. This would be a matter of convention. We certainly would like to think that "2+1=3" by virtue of something more substantial than its being a part of some arbitrary game.

The reason for accepting "2+1=3" as true is its connection with our procedure of counting. The first step is to count two objects. The "+1" is taken to mean that we are next to count one more object which was not included among the first two objects. The "=3" indicates that if we now count all these objects (the two objects and the one object) there will be three of them. These objects might be tastes, sounds or anything else which can be distinguished from each other. In the decimal system one can reduce arithmetical sums involving numbers greater than ten to this elementary procedure by using one object to represent ten objects.

In particular, we use the symbols "0" to "9" to represent anywhere from zero to nine objects. The same numerals in combination with the notion of a decimal place allow us to represent many objects. For example the "1" in "10" represents one unit of ten objects and the "2" in "235" represents two units of one hundred objects. When we add the two numbers, as in the following sum, our procedure is facilitated by the fact that it is handled by three simple equations, namely "5+0=5", "3+1=4" and "2+0=2":

$$\begin{array}{r} 235 \\ \underline{10} \\ 245 \end{array}$$

Hence, all arithmetical sums are reduced to very simple equations whose truth depends upon some basic facts of counting.

Let us now examine the possibility of changing our counting in such a manner that it would support arithmetical sums other than those of our own arithmetic. The first case which comes to mind is a situation in which objects to be counted do not behave normally. Let us say that every time we attempted to draw exactly three lines, four lines would happen to appear. Should we then count "1,2,4,5..."? This appears to support an arithmetic in which the equation "2+1=4" would be true.

I do not believe that this unusual situation would demonstrate an occasion in which ordinary arithmetic is wrong. In the case under consideration we should say

there are four lines because of some pattern such as ////
From this pattern we can see that 2+1 =3 since we will count three lines if we count the first two lines and then the one which follows it. If we interpreted "+" in "2+1=3" to mean something such as the operation of physically introducing new objects to those which were previously counted, then under the present context we might have reason to say that 2+1=4.

Another possibility which we have to consider is the case of counting strokes in the following fashion:

1 2   3
/ / / /

This type of counting would appear to result in an arithmetic in which the sum 2+1 is undefined. This method of counting is not essentially different from our own. I would like to illustrate this point by means of a discussion with an imaginary person who counts in the manner described above.

If we faced this person we could ask him what was wrong with counting the strokes as:

1 2 3 4
/ / / /

He would point out that we were mistaken in placing "3" at this location. We should have left a blank where we placed our "3" and placed "3" where we placed our "4". according to him. Thus, he associates a blank with the third stroke just as we associate the numeral "3" with it.

Even under the assumption that there will be other blanks associated with other strokes, the blank which we have been considering can be identified by the fact that it is the blank which occurs between the numerals "2" and "3" or, more precisely, it is the blank which is associated with the stroke which follows the stroke numbered "2" and precedes the strokes numbered "3".

What we are left with is a system of counting wherein the numerals "1" and "2" correspond to the numbers 1 and 2 while the blank with the aforementioned description corresponds to the number 3 and the numeral "3" corresponds to the number 4. It is now apparent that this manner of counting is not different from our counting in any respect more significant than counting in Italian is different from counting in French. Consequently this method of counting supports the same arithmetic as ordinary arithmetic.

The proponent of this other method of counting might object to the fashion in which I associated his blank with the third stroke by saying "I intended to disregard the blank space. After all, that is why I left it blank. It is not fair to turn it into some numeral by associating it with a stroke, turning it into something." One would like to respond to this with the observation that the only way the blank can be treated as nothing is to treat the third stroke as nothing. If this person allows us to rub out the third stroke from his pattern of $\overset{1}{|}\;\overset{2}{|}\;|\;\overset{3}{|}$, we would then be left with our usual pattern of

1 2 3
/ / /.

On the other hand, should he object to our rubbing out the third stroke his objection must be based on some reason such as that we have no right to ignore the third stroke since it is one of the strokes which was to be counted, we must point out the fact that this is exactly what he is doing. His insistence that the blank is a nil is tantamount to not counting the third stroke.

The next alternative to ordinary counting which we shall deal with is given by the following pattern:

1 2 3 4 5 6
/ / /   / .

We may briefly note that this situation is the complement of the one which we previously considered. In that case we encountered a situation where there were apparently more strokes than numerals while in the present situation there are apparently more numerals than strokes. In this case it appears that 2+1=345 or 2+1=3 or 2+1=4 or 2+1=5. This situation is actually quite simple in that what we have are four numerals, namely "345", "3", "4", and "5", for the number 3. It does not matter whether one or more of these numerals are used to designate 3. We see no problem in the fact that there are many ways of writing

"3" and not to mind that everyone does not have the same handwriting with respect to numerals as well as letters. Thus in this system one or more of the statements, depending on which numerals are chosen to represent 3, "2+1=345", "2+1=3", "2+1=4", "2+1=5" will each have the same meaning as "2+1=3" does in ordinary arithmetic.

The last form of counting which we shall consider is that given in the pattern

1 2 2 3
/ / / / .

Here it appears that 2+1=2 and 2+1=3. As the pattern stands we may distinguish one "2" from the other for we can say that one "2" is followed by a "2" while the other is followed by a "3". Hence we may name them respectively as "2left" and "2right". The first will correspond to our "2" while the second would correspond to our "3". Similarly the "3" of this system corresponds to our "4". Hence the first equation states that 2left+1=2right (2+1=3) while the second equation states that 2right+1=4 (3+1=4).

A proponent of this method of counting might wish to establish that it is exactly the same number that is being associated with these two strokes by changing the pattern to

1 2 3
/ / / / .

To prevent us from using "2" as one numeral and "2" as another numeral he may tell us that the connecting lines beneath the "2" were put there for our benefit and are not necessary for one who knows how to count properly.

We may now change our previous objection so that it will account for the new pattern. Previously, we had two "2"'s which we could spatially distinguish from each other. We can accomplish the same end by now referring to the distinguishableness of the strokes. The numeral which belongs to the second stroke has the characteristic of belonging to a stroke which is followed by a stroke which is assigned the same numeral. The numeral which belongs to the third stroke has the characteristic of belonging to a stroke which is followed by a stroke which is assigned a different numeral. Since these numerals possess different characteristics, they are not the same numerals and can in fact be identified as "2left" and "2right". Again, these two numerals are equivalent to our "2" and "3" respectively.

The reasoning which we have offered in support of rules of addition suggests arithmetic is not a matter of convention. We do not say that "2+1=3" is true because of the fact that people have agreed in making it "true". The truth of "2+1=3" depends upon its connection with counting, thus we do not have to reach any kind of agreement before we can evaluate the truth of the equation.

We may now see that Wittgenstein's view that following a rule is a practice does not commit him to conventionalism. When we judge that some statement is conventional we are saying in essence that it is a matter of agreement that it is true. There are two types of agreement. When people explicitly agree on the truth of a statement they are basically saying "let us decide whether this statement is true". There is another type of agreement, implicit in nature, which is agreement in those actions which constitute the practice of following a rule.

> "So you are saying that human agreement decides what is true and what is false?" - It is what human beings say that is true and false; and they agree in the language they use. That is not agreement in opinions but in form of life.[21]

This implicit agreement is thus not one that is based on a decision. Corresponding to these two types of agreement there are explicit and implicit versions of conventionalism. In any event, when a statement is described as conventional the crucial concept involved is "true by agreement". Hence when a statement is described as conventional it is presupposed that the description is an application of the rule "pick a statement that is true by agreement".

The point which Wittgenstein made in regard to the student and the rule "+2" applies to the rule mentioned above. That is to say, following the rule "pick a statement that is true by agreement" is a practice.

Following this rule is a practice and, as we have seen in the case of "2+1=3", not an explicit convention. If, as seems to be the case, this must apply to any rule for picking out explicit conventions, then no such rule can be correctly applied to itself. Hence, Wittgenstein's view that following a rule is a practice certainly is not that of an explicit conventionalist for according to such a view explicit conventionalism is self refuting.

Wittgenstein is certainly an implicit conventionalist insofar as he maintains that the following of a rule is a practice. This does not mean that we have to examine the implicit agreement which constitutes a practice before we can determine that "2+1=3" is true. It is this practice which enables us to apply the rules which we have.

At this time I would like to retrace my steps. I have first tried to show that equations such as "2+1=3" are not dependent upon a convention for their truth.[22] One can supply reasons for having the rules of addition which we find in arithmetic. These reasons are not that people have agreed in accepting these rules. Furthermore, I have argued that the fact that Wittgenstein maintained that the following of a rule is a practice does not show that Wittgenstein was committed to explicit conventionalism.[23]

FOOTNOTES TO CHAPTER ONE

[1] John Locke, An Essay Concerning Human Understanding, Bk.IV, ch.VII, sec.9.

[2] ibid., Bk.IV, ch.XVII, sec.8.

[3] George Berkeley, "A Treatise Concerning The Principles of Human Knowledge", sec.13.

[4] ibid., sec.16

[5] Euclid, The Thirteen Books Of Euclid's Elements, Bk.I, prop.9.

[6] Immanuel Kant, Immanuel Kant's Critique Of Pure Reason, p.577.

[7] loc.cit.

[8] ibid., p.182.

[9] loc.cit.

[10] ibid., p.579.

[11] loc.cit.

[12] Ludwig Wittgenstein, Philosophical Investigations, §§330-332.

[13] ibid., §§ 185-188. Wittgenstein uses "series" rather than "sequence".

[14] ibid., § 186.

[15] ibid., § 188.

[16] ibid., § 201.

[17] ibid., §§ 224,225; Remarks p.184,no.32.

[18] Wittgenstein, Philosophical Investigations, § 201.

[19] ibid., § 208.

[20] ibid., § 240.

[21] ibid., § 241.

[22] This argument is not intended to be a representation of Wittgenstein's position.

[23] Although this argument is my own, I believe it is consistent with Wittgenstein's position (Remarks p.96, no. 70; p.96f., no.72; p.98f., no.75; p.184f., no.33).

CHAPTER TWO

THE PAUCITY OF PARADISE

(A) Cantor's Diagonal Method

Wittgenstein provides an extremely interesting analysis of the theorem that the real numbers are uncountable. Cantor published the proof of this in 1874.[1] Here is a sketch of the popular diagonal method of proof:

Assume that the real numbers are countable. They may then be listed in the following manner.

$$\begin{array}{cccc} a_{11} & a_{12} & a_{13} & \cdots \\ b_{21} & b_{22} & b_{23} & \cdots \\ c_{31} & c_{32} & c_{33} & \cdots \\ \vdots & \vdots & \vdots & \end{array}$$

If $a_{11} \neq 0$, we set $d_1=0$. If $a_{11}=0$, we set $d_1=1$.

If $b_{22} \neq 0$, we set $d_2=0$. If $b_{22}=0$, we set $d_2=1$.

Similarly, if the nth place of the nth row $\neq 0$, we set $d_n=0$ and if the nth place of the nth row $= 0$, we set $d_n=1$. The real number $D=d_1.d_2d_3\ldots$ will not appear, for any integer n, in the nth row. This contradicts the fact that the above countable list contained all real numbers. Therefore the set of real numbers is uncountable.

Wittgenstein was of the opinion that the statement of the theorem, considered by itself, could be very misleading. He strongly advocated the procedure of letting the proof give meaning to the statement of the theorem.

> The result of a calculation expressed verbally is to be regarded with suspicion. The calculation illumines the meaning of the expression in words. It is the finer instrument for determining the meaning. If you want to know what the verbal expression means, look at the calculation; not the other way about (Remarks p.54,no.1).

Wittgenstein emphasizes the fact that the diagonal procedure is very simple and may be considered within the framework of school-children without any set theory. They may be given a very long list of numbers in décimal form and be required to write down a number in decimal form that is different from all the numbers in the given list. The student will then implement the diagonal method and come up with an appropriate answer. This method, viewed under the above circumstance, "...changes the aspect of Cantor's discovery" (Remarks p.56,no.3). We are not committed to believe "...we have discovered an enormously large set of objects, one which transcends in magnitude even the rational numbers."[2]

Wittgenstein states that the notion of discovery in the proof should be replaced with the view that the proof determines and gives birth to a concept (Remarks p.56,no.3). He places stress upon the fact that Cantor's diagonal method does not actually give us a real number that is not in the countable listing of real numbers assumed in the proof. Cantor makes it meaningful to speak of a number that is different from the countable listing.

> Cantor could say: You can prove that a number is different from all the numbers in the system by proving that it differs in its first place from its first number and in its second place from its second number and so on (<u>Remarks</u> p.58,no.6).

The diagonal number is, in effect, a technique. It is a technique which applies to other techniques. This doctrine is a result of Wittgenstein's theory that real numbers in decimal form are methods of construction (<u>Remarks</u> p.55,no.2; p.59,no.9).

The diagonal as a technique or method applied to other methods leads to an intriguing separation.

> But we cannot very well say that the rule of altering the places in the diagonal in such-and-such a way is as such proved different from the rules of the system, because this rule is itself of 'higher order'; for it treats of the alteration of a system of rules, and for that reason it is not clear in advance in which cases we shall be willing to declare the expansion of such a rule different from all the expansions of the system (<u>Remarks</u> p.58,no.7).

The implication from this is that the properties which apply to one order may have no meaning when applied to another order. The transfer from one order to the other cannot be taken for granted. In our case, the diagonal calculation cannot be compared with the expansions of the original list without justification. I believe that this explains Wittgenstein's strong remarks against the meaningfulness of the concept of a series of real numbers.

> So if it is asked: "Can the real numbers be ordered in a series?" the sure answer might be: "For the time being I can't form any precise idea of that (Remarks p.55,no.2).

A new concept must be supplied before the connection between a series (countable sequence) and the real numbers can be made.

> To put it better, I have got certain analogous formations, which I call by the common name 'series'. But so far I haven't any certain bridge from these cases to that of 'all real numbers'. Nor have I any general method of trying whether such-and-such a set 'can be ordered in a series' (Remarks p.55,no.2).

When this bridge is constructed we have a choice as to the theorem that is proved by the proof. "'These considerations may lead us to say that $2^{\aleph_0} > \aleph_0$ '" (Remarks p.58,no.8). We may also conclude that the real numbers and the natural numbers cannot be compared in magnitude.[3] The proof may indicate that real numbers and natural numbers are very different concepts and that it is a mistake to compare the magnitudes of the two sets (Remarks p.57,no.3). The customary conclusion that the cardinality of the continuum is greater than $\aleph_0$ should not be looked upon as an obvious conclusion from the proof. "It is not simply a fact read off from observation, comparable to the fact that there are more geese than swans in the pond."[4]

It is not surprising that a proof, by itself, may lead to very different conclusions. An appealing example of this is the one-one correlation of the natural numbers with the even natural numbers. This contradicts the apparently unassailable fact that there are definitely more natural numbers than even natural numbers. Every even natural number is a natural number and there are many natural numbers that are not even. The whole is greater than the part. This leads one to conclude that there are no infinite sets.[5]

Dedekind saw no harm in this one-one correlation. He took it as the defining characteristic of an infinite set. "A system S is said to be infinite when it is similar to a proper part of itself..."[6] This was truly a creation of a concept in that anyone following Dedekind's method does not have to stop at the contradiction of the previous paragraph but may use it as a first step into a new realm of mathematical activity. The same one-one correlation is considered by some to be a proof that infinite sets do not exist, but is viewed by others as an indication that infinite sets do exist.

We cannot look at the proof by itself. "The motto here is: Take a wider look round" (<u>Remarks</u> p.54,no.1). When this wider look is taken we will be in a position to grasp the concepts that are required to pass from the proof to the theorem. This is an illuminating approach from the standpoint of the philosophy of mathematics. Mathematical results are not to be discarded; we keep them and investigate them with the hopes of seeing what new concepts are being constructed. With this in mind, I should like to compare Wittgenstein's views on the real numbers and "$2^{\aleph_0} > \aleph_0$" with the mathematical material found in Suppes' Axiomatic Set Theory. I have chosen this latter work since it provides a very thorough treatment of the real numbers and relates them to their decimal expansions. The real numbers, within a set theoretic framework, are generated from the rationals by means of either Cauchy sequences or Dedekind cuts. In this chapter we will use Cauchy sequences. (In the following chapter on Dedekind's theorem we will, of course, employ the device of Dedekind cuts).

A Cauchy sequence of rational numbers is a sequence X, of rational numbers, such that for every rational $\epsilon > 0$, there is a natural number N such that for every m, $n > N$ $|x_n - x_m| < \epsilon$ where x_m is the mth member of the sequence X.[7] The next definition establishes the relation of Cauchy equivalence, $\simeq_c$, between two Cauchy sequences of rationals. $X \simeq_c Y$ if and only if for every positive rational number ϵ there is an integer N such that for every $n > N$ $|x_n - y_n| < \epsilon$.[8] The equivalence class of a Cauchy sequence, X, of rational numbers is defined as: $[X]_r = \{ y : y$ is a Cauchy sequence of rational numbers & $y \simeq_c X \}$.[9] The real numbers are then defined: $R_e = \{ y : (\exists x)\ x$ is a Cauchy sequence of rational numbers & $y = [X]_r) \}$.[10] The following theorem states that our set theoretic real numbers correspond to decimal (if we take r=10) expansions of the form $a.d_1 d_2 \ldots d_n \ldots$

Let r be an integer ≥ 2. Every real number x is uniquely representable with respect to the radix r as a sequence $a, d_1, d_2, \ldots d_n, \ldots$ such that

(i) a is the largest integer equal to or less than x,

(ii) for all n, $0 \leq d_n < r$ and d_n is an integer,

(iii) it is not the case that there is an N such that for $n > N$, $d_n = r-1$,

(iv) the sequence whose terms c_n are defined recursively by $c_o = a$

$$c_{n+1} = c_n + d_{n+1} / r^{n+1}$$

is a Cauchy sequence which converges to x.[11] We also have the result that every decimal expansion corresponds to a particular set theoretic real number.

Given a countable sequence of real numbers, the preceding theorem permits the translation of these real numbers into decimal form. The rule for constructing a real number from the diagonal of the countable sequence results in a decimal expansion. This rule is of higher order than the real numbers in the sequence.

The claim that the rule for constructing the diagonal is of higher order than the rules which govern the real numbers in our original list is based on Russell's ramified theory of types. According to this aspect of Russell's theory of types two propositional functions (taken as attributes rather than expressions) are of different orders if the linguistic expression which names one of the functions contains a bound variable whose range consists of the extension of the other propositional function.[12] Let us now see how this affects the diagonal proof.

Let M be any countable sequence of real numbers M_n, $n \in \omega$, in decimal notation. For reasons of simplicity let us take these real numbers as being of the form $.a_1a_2a_3\ldots$ That is to say, each member M_n of M is a countable sequence of numbers a_m such that $9 \geq a_m \geq 0$. Since a sequence is properly speaking a function whose domain is the natural numbers, each M_n is a function from ω into $\{0,1,2,\ldots,9\}$. The diagonal real number d is also such a function and is defined as follows: $d = \{\langle n,s\rangle : n \in \omega \ \& \ s \in \{1,2\} \ \& \ (\forall x)(\forall n)[(x \in M \ \& \ n \in \omega \ \& \ x = M_n) \rightarrow ((M_n(n) \neq 1 \& s=1) \vee (M_n(n)=1 \& s=2))]\}$. We may look upon M and d as propositional functions whose arguments are ordered pairs of the forms $\langle n,M_n\rangle$ and $\langle n,s\rangle$ respectively. It will be noted that in the definition of d we quantified over every thing which is an M, namely the extension of M. This is evident in the underlined parts of the definition: $d = \{\langle n,s\rangle : n \in \omega \& s \in \{1,2\} \& (\underline{\forall x})(\forall n)[(\underline{x \in M} \ \& \ n \in \omega \ \& \ x = M_n) \rightarrow ((M_n(n) \neq 1 \& s=1) \vee (M_n(n)=1 \& s=2))]\}$. Thus, according to the ramified theory of types, d is of higher order than M.

Despite the fact that this rule is of higher order than the real numbers in the sequence we may still compare the real number that results from the diagonal method with those of the sequence. This comparison requires the axiom of Extensionality which tells us that any set is determined solely by its members without regard to the order of the defining expression. Cantor's diagonal method results in a sequence of the form $.d_1d_2d_3...$ This sequence is of exactly the same form as any decimal expansion in the original list. The sequences are sets and may therefore be compared with each other independently of the rules which led to their formation. It is very simple to prove that the diagonally constructed sequence differs from every decimal expansion in the original sequence once we can restrict ourselves to the members of the decimal expansions. Everything is then translated back into the form of our set theoretic real numbers.

This uncovers the fact that one must not take an intensional view of the real numbers if the proof in question is to be convincing. An intensional consideration could lead to the difference of order mentioned by Wittgenstein. The point to be understood here is that there is nothing in the diagonal method as such to enforce the adoption of the extensional treatment of the real numbers. The Axiom of Extensionality is thus a part of the mathematical surroundings of Cantor's diagonal proof (Remarks p.54,no.1).

It was also assumed in the proof that there is the set of real numbers. This fact depends on the Power Set and Replacement axioms applied a number of times to the set of integers whose existence is guaranteed by the Axiom of Infinity. These procedures are necessary to disarm the attack, "There is no system of irrational numbers - but also no super-system, no 'set of irrational numbers' of higher-order infinity"(Remarks p.58,no.7). These are by no means trivial additions to the diagonal method. Without this background of set theory the diagonal method proves very little.

There is no doubt that Wittgenstein was correct in his comments on the simplicity of the diagonal method. He is not saying that set theory should not be added to the diagonal method. His wish is that we look at the grounds for asserting a proposition such as "$2^{\aleph_0} > \aleph_0$" and that we do not let such a proposition float in the air with no support (Remarks p.58f.,no.8). Since set theory is a part of mathematics according to Wittgenstein (Remarks p.134,no.5;p.137,no.7) we shall study the manner in which Wittgenstein looked upon this subject. His comments are mainly about the concept of infinity in set theory but this will indicate his attitude towards the whole subject since all the controversial areas of set theory are rooted in the notion of infinity.

(B) The Mathematical Use Of "Infinity"

Wittgenstein emphasized the vagueness that surrounds most applications of "infinity" and "the infinite". Mathematics must not presuppose that there is a clear meaning to the word that is suited for mathematical use, for there is none. "'Ought the word 'infinite' to be avoided in mathematics?' Yes; where it appears to confer a meaning upon the calculus; instead of getting one from it" (Remarks p.63,no.17). It is a mathematical task, therefore, to provide a clear meaning for the word. The meaning of the word 'infinite' is not clear unless one has an employment of the word. Since there is no employment of expressions involving the various infinities, the mathematician must invent such an employment (Remarks p.59,no.9).

Wittgenstein considers any intuitive picture as being inadequate to support a proposition such as "the fractions cannot be arranged in a series in order of magnitude". An attempt to form such a picture results in a vision of a multitude of fractions constantly and endlessly issuing forth between any two fractions which may have been grasped. This picture can easily make us dizzy (Remarks p.60,no.11). Wittgenstein suggests that there is a cure for this dizziness which will restore our equilibrium. He prescribes that "...we shall fall back on the technique of calculating fractions, about which there is no longer anything queer" (Remarks p.60,no.11).[13]

This is a very elegant solution. A simple technique captures the meaning of the proposition. Given any two fractions $\frac{a}{b}, \frac{c}{d}$ where $\frac{a}{b} < \frac{c}{d}$, the fraction $\frac{da+bc}{2bd}$ is such that $\frac{a}{b} < \frac{da+bc}{2bd} < \frac{c}{d}$. This illustrates the fact that there is no next largest fraction. There is nothing to impart dizziness in this technique; contrary to the picture which is conjured up by the wording of the theorem. Wittgenstein supplies a similar analysis of the theorem that the fractions can be ordered in a countable sequence. One learns this theorem by learning a new calculation which assigns to each fraction a natural number (Remarks p.61,no.15).

	1	2	3	4	. . .
1	1	3	6	10	
2	2	5	9	.	
3	4	8	.		
4	7	.			
.	.				

There is no more to the theorem than the calculation given in the preceding diagram (other calculations will also accomplish the ordering). Our interest is to be focused upon the calculation. The wording of the theorem associates a memorable picture to the calculation. Beyond this association, the proposition stating the theorem has nothing to offer (Remarks p.62f.,no.16).

Unfortunately, Wittgenstein did not analyze the role of infinity in mathematics in terms of a particular calculation as he did with the theorems stating that the reals are uncountable and the fractions are countable but not ordered according to magnitude. He does not object to employing the symbol " $\aleph_0$ " for we have a grammar for its use.

> This is connected with the fact that among the calculi of mathematics we have a technique which there is a certain justice in calling "1-1 correlation of the members of two infinite series", since it has a similarity to such a mutual correlation of the members of what are called 'finite' classes (Remarks p.59,no.9).

This does not justify everything in set theory as Wittgenstein proceeds to question Frege's definition of an infinite number. Wittgenstein does not assume that the 1-1 correlation spoken of in the above quotation 'gives sense to Frege's definition of the number $\aleph_1$ as the class of all classes that are equinumerous with the class 'infinite series.'[14] Until a use for the expression is invented Wittgenstein puts it on the same level as the expression "class of all angels that can get on to a needlepoint" (Remarks p.59,no.9).

A use for the symbol "$\aleph_1$" has been invented. Frege's definition was discarded on account of the paradox which it entailed. The closest system to Frege's is that found in Principia Mathematica[15] where an elaborate theory of types supports a modified view of Frege's cardinal numbers. There are certain rules which we can state that give meaning to the symbol "$\aleph_1$" in set theory. Let us deal with the problem in the context of Zermelo-Fraenkel set theory.

We may start off with the Axiom of Infinity. Some difficulty may accompany this axiom if we only look upon it as declaring that an infinite set exists. Wittgenstein felt that from the normal understanding of the word "infinity" one would not find anything that was truly infinite in set theory:

> This way of talking: "But when one examines the calculus there is nothing infinite there" is of course clumsy - but it means; is it really

> necessary here to conjure up the picture of the infinite (of the enormously big)? And how is this picture connected with the calculus? For its connexion is not that of the picture |||| with 4.
>
> To act as if one were disappointed to have found nothing infinite in the calculus is of course funny; but not to ask: what is the everyday employment of the word "infinite", which gives its meaning for us; and what is its connexion with these mathematical calculi?" (Remarks p.63,no.17).

Wittgenstein is obviously correct in that there is no simple picture to go along with "$\aleph_0$" or "infinity" as there is a picture to correspond with "4". There is no convincing, accepted picture of the enormously big. Even if there were such a picture it would not be necessary to use it in connection with our set theory.

Do we require a picture of the infinite to comprehend the meaning of the formula "$(\exists A)[0 \epsilon A \,\&\, (\forall B)(B \epsilon A \rightarrow B \cup \{B\} \epsilon A)]$"? One of the things which we have here is an operation which assigns $B \cup \{B\}$ to B, that is to say it takes us from a number to its successor. This is not something to be marvelled at. It is a very simple operation and does not carry with it any hugeness. One may compare such an operation with multiplication. A child may be said to have learnt $\aleph_0$ multiplications without having learnt anything huge.[16] "The teacher does not say to himself, 'Ah, fancy these boys of ten and eleven having such vast knowledge!'"[17] The child has merely become proficient in the employment of some elementary rules. The rules are sufficiently wide in scope to allow for many instances of multiplication to be performed. This is similar to the observation which we made in Chapter One regarding the universality of a proof involving a geometrical construction where the rules of construction may be applied to all angles, triangles, etc.

This takes care of the successor operation but we must still examine the role of the existential quantifier in the statement "$(\exists A)[0 \in A \,\&\, (\forall B)(B \in A \rightarrow B \cup \{B\} \in A)]$". Wittgenstein is suspicious of the word "exist" in mathematical contexts (Remarks p.60,no.11). It seems that there is more than a rule or technique here since the statement asserts that there is a collection which consists of 0 and every successor but, in fact, there is no very large entity in this case. Wittgenstein made the same point in regard to infinite decimals which he viewed as part of an endless technique and not as some gigantic extension (Remarks p.144,no.19). An endless technique is one where there is no assigned last step. Wittgenstein describes such processes in a fashion which makes room for a further development that is a completion of the technique.

> To say that a technique is unlimited does not mean that it goes on without ever stopping - that it increases immeasurably; but that it lacks the institution of the end, that it is not finished off. As one can say of a sentence that it is not finished off if it has no period. Or of a playing-field that it is unlimited, when

> the rules of the game do not
> prescribe any boundaries -
> say by means of a line
> (<u>Remarks</u> p.60f.,no.12).

The Axiom of Infinity, in asserting the existence of a certain set, is effectively extending the applicability of the successor operation in that we can now go beyond the technique of forming the iterated successors of 0. Prior to this axiom it would be ridiculous to look for anything that came after all the integers. The axiom gives sense to the statement " $\aleph_0$ is greater than any integer". We are now given a rule that allows other rules or techniques to be applied to the operation which gives us the integers. For example, we may take the set of ordered pairs of integers or the power set of all the integers. This situation is comparable to that encountered in Wittgenstein's study of the diagonal method where there was no apparent ground for comparing a rule of higher order, the diagonal method, with a system or rule of lower order. Set theory provided the framework for comparing the two processes by placing them, in effect, on the same plane.

An employment of "$\aleph_1$" can be constructed by implementing other techniques to $\aleph_0$. The Power Set Axiom is one example of a technique which, when applied to $\aleph_0$ will give sense to the proposition that there is a cardinal greater than $\aleph_0$. Cantor's theorem that $\overline{\overline{\mathcal{P}A}} > \overline{\overline{A}}$ accomplishes this. The proof that every set of ordinals has a least member gives meaning to the statement "the least cardinal greater than $\aleph_0$ ". Instead of Cantor's theorem one might apply the proof that there is no greatest cardinal to show that there is a cardinal greater than $\aleph_0$. In any event, there is nothing large in these constructions. The Power Set Axiom is a rule that gives free rein to the operation $x \subseteq y$. Some subsets of a set may be of a very intricate nature, nevertheless this does not detract from the fact that they still can be viewed as x's such that $(\forall y)(y \in x \rightarrow y \in z)$ where x is a subset of z. Again the analogy should be drawn with an elementary geometrical construction. The rules for the construction are quite simple and easy to grasp though particular instances of the construction may be extremely difficult to put into practice, eg. the bisection of an angle of 10^{-20°.

Although I have constantly placed great weight upon the simplicity of a number of set theoretical processes, I do not want to leave the impression that they can be taken for granted. Their novelty and originality give birth to new mathematical studies. Whenever a new dimension is added to mathematics there is a creative invention that gives it meaning (Remarks p.140,no.11).

Wittgenstein definitely did not want us to reject the Axiom of Infinity and other set theory involving "the infinite". He made a very important distinction between mathematics and the discussion of mathematics.

His work was intended to be entirely confined to the latter area.

> What I am doing is, not to shew that calculations are wrong, but to subject the interest of calculations to a test. ...Thus I must say, not: "We must not express ourselves like this", or "That is absurd", or "That is uninteresting", but: "Test the justification of this expression in this way" (Remarks p.63,no.18).

This passage indicates that Wittgenstein did not object to the word "infinite" in mathematics. In his analysis of "infinite" he discovered that it is used differently in mathematics and ordinary life. The thought which he brought to the foreground is that in ordinary life "the infinite" carries with it a picture that involves hugeness whereas such a picture is inappropriate to "the infinite" in mathematics. This is the reason for Wittgenstein's insistence that mathematics is to provide some meaning to the word. Once this is achieved - we have seen that it is achieved - Wittgenstein does not insist that the words should have the same meaning both within and without mathematics. The mathematician should not be accused of not dealing with anything that is infinite for the mathematician may say "This is infinite."[18] In another lecture Wittgenstein stated, "I am not saying transfinite propositions are false, but that the wrong pictures go with them."[19]

Wittgenstein has, however, been associated with the doctrine of strict finitism. In the next section we shall examine the reasons on which this association has been proposed.

(C) Strict Finitism

First of all it is necessary to describe the doctrine of strict finitism. Strict finitist mathematics consists of mathematical constructions or configurations which can actually be surveyed.[20] The distinction between finitism and strict finitism occurs in the word "actually" of the preceding sentence. Finite mathematics includes within it any multiplication of two numbers, provided the numbers in question are finite. The answer to such a multiplication will be another finite number. All of the entities and processes of finite mathematics are finite, therefore the results will be finite as in the case of multiplication. As a result of this, every statement of finite mathematics is decidable in the sense that for each of these statements there is a procedure and a natural number n such that after n steps of the procedure one will have either proved or disproved the given statement.

The strict finitist insists that the finitists restriction of mathematics to finite entities and procedures is not adequate. The strict finitist believes that mathematics should concern itself only with those finite entities which can in fact be apprehended. Similarly, the only permissible procedures are those which one can as a matter of fact complete. The strict finitist for example would not call a sequence of one million digits a numeral since it is too long for us to actually survey it or take it in. A strict finitist would also not permit such notions as the number which is the product of the first billion prime numbers since one cannot actually go through the many steps required to calculate this number. Thus, the statements that are found in strict finitist mathematics are decidable in a number of steps which can actually be performed.

Strict finitism is clearly a revisionary theory of mathematics in that it rejects a considerable portion of mathematics. The use of infinite sets in set theory and analysis makes these areas unacceptable to a strict finitist.

We shall now present Kielkopf's argument for his claim that Wittgenstein's comments on Cantor's diagonal argument were motivated by Wittgenstein's strict finitism.[21] The argument will be given in two parts(I and II).

I: Kielkopf refers to (<u>Remarks</u> p.153,no.41) which states that

> Concepts which occur in 'necessary' propositions must also occur and have a meaning in non-necessary ones.

In association with this remark Kielkopf mentions (<u>Remarks</u> p.186,no.35) where Wittgenstein declares that there is no concept of which we can assert $2^{\aleph_0}$ empirically. Kielkopf interprets these passages as suggestions to the effect that $2^{\aleph_0}$ is mathematically useless since it has no empirical use.

II: Wittgenstein thought it a mistake to use a picture of the enormously big to give meaning to a mathematical proposition (Remarks p.63,no.17). If we follow Wittgenstein's suggestion, in (Remarks p.57,no.5; p.59,no.9; p.61,nos.13,14), to apply "infinite" in mathematics only in connection to techniques such as counting for which there is no final step, we will avoid the mistake mentioned in the preceding sentence since "infinite" will now get its meaning from mathematical language rather than give meaning to mathematical statements.

From the above, Kielkopf draws the conclusion that:

> ...Wittgenstein wrote as if talking about infinite pluralities in mathematics is objectionable because it leads us to poor strict finitistic mathematics,...So I do not think it rash to conclude that Wittgenstein objected to talk of actual infinities on strict finitistic grounds. Thus we can regard his objections to Cantor's theorem as stemming from the strict finitism in his thought.[21]

I would now like to discuss Kielkopf's argument I with the purpose of showing that Wittgenstein was not a strict finitist. We should be aware of the fact that the doctrine, mentioned in I, that empirically useless statements are also mathematically useless is a strict finitist doctrine. The vast majority of set theory, analysis and other domains which employ a concept of infinity would be discarded since many of the concepts which they employ do not appear in non-necessary or empirical propositions. The number concepts in non-necessary propositions are applicable to matters of fact. It might well be that there is no need to speak of some very large finite numbers, let alone infinite numbers, to describe states of the world.

For example, it is likely that there is some very large finite number y, perhaps for some subatomic particle the number of these particles in the universe, such that y is the largest number that may appear in a non-necessary proposition. In this case we are left with strict finitism since y will be the largest number permitted to appear in mathematics while $y+1$ and y^{10} will not be considered proper numbers at all. Thus, if we accept Kielkopf's initial interpretation of the passages mentioned in I we are compelled to consider Wittgenstein as a strict finitist.

I do not think that we should accept the initial position from which Kielkopf argues. We will first note that he focuses in on only a particular aspect of (Remarks p.186,no.35), namely a part of the last (fifth) paragraph of this number. The first and last paragraphs of this remark are relevant to the discussion and are provided below.

> When I said that the propositions of mathematics determine concepts, that is vague; for '2+2=4' forms a concept in a different sense from '$p \supset p$', '$(x).fx \supset fa$', or Dedekind's Theorem. The point is, there is a family of cases.
>
> ...

> A number is, as Frege says, a property of a concept - but in mathematics it is a mark of a mathematical concept. $\aleph_0$ is a mark of the concept of a cardinal number; and the property of a technique. $2^{\aleph_0}$ is a mark of the concept of an infinite decimal, but what is this number a property of? That is to say: of what kind of concept can one assert it empirically?

It appears that Wittgenstein, in the last paragraph of the remark, is giving the reader a particular instance of the general observation he made in the first paragraph. Wittgenstein says in the first paragraph that some mathematical statements and proofs form concepts in a different manner than do other mathematical statements or proofs. In the last paragraph Wittgenstein provides two mathematical concepts, $\aleph_0$ and $2^{\aleph_0}$, one of which he believes is a property of a technique while the other, he thinks is not a property at all since it cannot be asserted of a concept empirically. This illustrates the fact that according to Wittgenstein a mathematical statement containing "$2^{\aleph_0}$" (which is not a property) determines a concept in a different manner than a mathematical statement that contains "$\aleph_0$" (which is a property).

The distinction which Wittgenstein makes in this passage suggests that he believed $2^{\aleph_0}$ is a concept which is empirically useless. It does not show that Wittgenstein believed that $2^{\aleph_0}$ is mathematically useless.

Kielkopf attributed this latter belief to Wittgenstein on the basis of this passage and (Remarks p.153,no.41). This last remark, quoted in I, is very strong and would lead to the conclusion that $2^{\aleph_0}$ is a concept which is useless both within and out of mathematics. It is certainly one which is very clear and cannot be interpreted away. I will attempt to show that the view expressed in this remark was not Wittgenstein's final position as there are a number of passages in Wittgenstein which are opposed to it. I hope to demonstrate that this remark is an isolated one whose acceptance requires the rejection of some of Wittgenstein's more important thoughts.

Perhaps the closest Wittgenstein came to echoing (Remarks p.153,no.41) in regard to set theory was (Remarks p.137,no.7):

> Imagine set theory's having been invented by a satirist as a kind of parody on mathematics. - Later a reasonable meaning was seen in it and it was incorporated into mathematics. (For if one person can see it as a paradise of mathematicians, why should not another see it as a joke?)
>
> The question is: even as a joke isn't it evidently mathematics? -
>
> And why is it evidently mathematics? - Because it is a game with signs according to rules?
>
> But isn't it evident that there are concepts formed here - even if we are not clear about their application?
>
> But how is it possible to have a concept and not be clear about its application?

The last two sentences of this remark, at first glance, appear to demolish the appropriateness of calling $2^{\aleph_0}$ a concept since $2^{\aleph_0}$ would not have a clear application if it was taken as part of a mathematical joke. This is not, however, the point which Wittgenstein is attempting to make. I believe that Wittgenstein, in this remark, wants to make the observation that set theory can be a part of mathematics despite the fact that it may be performed by a mathematician who gives it a ridiculous interpretation.

The preceding remark is connected with (Remarks, pp.134-136, no.5) where Wittgenstein compares fanciful applications of set theory with fanciful applications of $\sqrt{-1}$. A person who is mad might invent $\sqrt{-1}$ because he is taken up by the paradoxical nature of the idea and imagines that in calculating with $\sqrt{-1}$ he is operating with the impossible. Wittgenstein allows that such a person's calculations can be perfectly sound. It is the interpretation given to these calculations that are incorrect. Wittgenstein concludes (Remarks pp.134-136, no.5) with:

> In other words: if someone believes in mathematical objects and their queer properties - can't he nevertheless do mathematics? Or - isn't he also doing mathematics?
>
> 'Ideal object.' "The symbol 'a' stands for an ideal object" is evidently supposed to assert something about the meaning, and so about the use, of 'a'. And it means of course that this use is in a certain respect similar to that of a sign that has an

> object, and that it does not stand for any object. But it is interesting what the expression 'ideal object' makes of this fact.

I think it is fair to say, on the basis of this passage, that Wittgenstein considered $2^{\aleph_0}$ as one of these ideal objects in accordance with the description of "ideal object" which he gave in the preceding quotation. That is to say "$2^{\aleph_0}$" is used in a way that is similar to the use of a sign which has an object, though "$2^{\aleph_0}$" certainly does not stand for an object. Wittgenstein nevertheless admits that, as wrong as it is to view these ideal objects as objects, one who has this view can still do mathematics.

The point of view expressed in the last two remarks which we considered, (<u>Remarks</u> pp.134-136,no.5;p.137,no.7), would not require that a mathematical concept such as "$2^{\aleph_0}$" must also have an empirical application. This last requirement is that of (<u>Remarks</u> p.153,no.41). (<u>Remarks</u> pp.134-136,no.5;p.137,no.7) have in their favor the fact that they fit in with Wittgenstein's remarks on Cantor's diagonal argument. We are now in a position to account for the other references to Wittgenstein by Kielkopf without sharing the conclusions which he drew from them.

In argument II Kielkopf states that in (<u>Remarks</u> p.57, no.5;p.59,no.9;p.61,nos.13,14) Wittgenstein is suggesting that we apply "infinite" in mathematics just in those cases where we refer to techniques such as counting which have no final step. Of these four remarks, (<u>Remarks</u> p.59, no.9) is particularly relevant. As we have seen on pp.69f.,72ff. one of Wittgenstein's concerns in this remark is that we must invent a use for the various infinities in mathematics. This is not to say that we should not apply "infinite" to a concept such as $2^{\aleph}$. We agree with Kielkopf that the infinities of set theory are not empirical notions. It is for this reason that it is so important for Wittgenstein to stress that in making a statement about a set being uncountable (not denumerable) we are determining or forming a concept rather than describing a fact of nature (<u>Remarks</u> p.56,no.3). The use of an expression such as "$2^{\aleph} > \aleph_0$" does not accompany it as the use of "John is taller than Mary" accompanies this latter expression. The first expression must be given a meaning. One cannot assume that its meaning follows from the meaning given to "$3>2$".

Now, if we provide the employment of "infinity" as a mathematical term, it is clear that we will be giving "infinity" its meaning by way of mathematics. Thus, we will avoid the mistake to which Kielkopf made reference in II. We are doing this, however, in a manner which does not prevent one from including $2^{\aleph_0}$ as a legitimate part of set theory. We can also avoid the platonism spoken of in II without at the same time restricting our number expressions to observable totalities and our technique of counting. Those remarks of

Wittgenstein which we examined on pp.95-98 place the set theoretical calculus and the calculus surrounding $\sqrt{-1}$ on a similar level to the extent that both of these calculi can be interpreted as manipulations of signs which stand for very odd objects. In both cases the problem is not with the calculus. The platonistic objects arise from the misinterpretations which are imposed on the calculus.

The strict finitist philosophy of mathematics is a revisionary philosophy of mathematics as it rejects such concepts as "$2^{\aleph_0}$". Wittgenstein's procedure is to leave the mathematical calculi as they are. He wants to examine the connection between the everyday employment of "infinite" and the mathematical calculus (Remarks p.63,no.17). "What I am doing is, not to shew that calculations are wrong, but to subject the interest of calculations to a test" (Remarks p.63,no.18).

We conclude that Wittgenstein's argument against platonism and mathematics which gets its meaning from "infinity" as it is used out of mathematics, mentioned in II, are not the arguments of a strict finitist. Furthermore it was not Wittgenstein's belief that $2^{\aleph_0}$ was mathematically useless as stated in I. Thus, in disagreement with Kielkopf, we do not take Wittgenstein's comments on Cantor's diagonal method to be the comments of a strict finitist.

FOOTNOTES TO CHAPTER TWO

[1]George Cantor, *Contributions To The Founding Of The Theory Of Transfinite Numbers*, p.38ff.

[2]V.H. Klenk, *Wittgenstein's Philosophy of Mathematics*, p.51.

[3]*ibid*., p.52.

[4]*loc.cit*.

[5]James Thomson, "Infinity In Mathematics And Logic", p.184f.

[6]Richard Dedekind, "The Nature And Meaning Of Numbers", p.63.

[7]Patrick Suppes, *Axiomatic Set Theory*, p.175.

[8]*ibid*., p.178.

[9]*ibid*., p.181.

[10]*loc.cit*.

[11]*ibid*., p.189.

[12]The initial motivation for the theory is found in A.N. Whitehead and Bertrand Russell, *Principia Mathematica*, I, 37-65, 161-167. The theory is clarified and criticized in F.P. Ramsey, "The Foundations Of Mathematics", pp.172-200 and W.V.O. Quine, *Set Theory And Its Logic*, pp.244f., 249-258.

[13]I became aware of the connection between general mathematical propositions and rules or techniques in Wittgenstein's thought through V.H. Klenk, *Wittgenstein's Philosophy of Mathematics*, p.103.

[14]Gottlob Frege, *The Foundations of Arithmetic*, pp.79f., 96. Frege's definition of ∞_1, defines $\aleph_0$. Similarly the description of ∞_2 will match $\aleph_1$.

[15]A.N. Whitehead and Bertrand Russell, *Principia Mathematica*, II,4.

[16] Ludwig Wittgenstein, Wittgenstein's Lectures on Foundations of Mathematics, p.141f.

[17] ibid., p.256.

[18] ibid., p.255.

[19] ibid., p.141.

[20] Georg Kreisel, "Wittgenstein's Remarks On The Foundations Of Mathematics", p.148f.

[21] Charles Kielkopf, Strict Finitism, p.137f. Klenk has argued in her Wittgenstein's Philosophy of Mathematics, pp.92-118, that Wittgenstein was not a strict finitist. My discussion is directed towards Kielkopf.

[22] ibid., p.138.

CHAPTER THREE

DEDEKIND'S THEOREM

Wittgenstein's comments on Dedekind's theorem are among the most difficult to understand in Remarks On The Foundations Of Mathematics. We shall deal with these comments in three parts. In Part A Dedekind's theorem is presented. Part B will concern itself with Wittgenstein's comments on Dedekind's system and its relation to the number line. Part C is an inquiry about the nature of a Dedekind cut.

(A) Dedekind's Theorem

The basic properties of the class of rationals, Q, shall be assumed. Prior to introducing the concept of a cut it is helpful to see how one can use a rational number to form a separation of the rationals.

Let a be any rational number. Two infinite sets of rationals can be defined in terms of a. The first set A_1 consists of all rationals less than a while the second set A_2 consists of those rationals greater than a. It does not matter whether we include a in A_1 or in A_2. In the first case a will be the greatest number in A_1. In the second case it is the least number in A_2. In any event Q is separated into two sets and every member of A_1 is less than every member of A_2.[1]

A cut of the rational numbers is defined as a pair of non-empty, jointly exhaustive sets A_1, A_2 of rational numbers such that every member of A_1 is less than every member of A_2. The cut is denoted "(A_1, A_2)".

> We can then say that every rational number a produces one cut or, strictly speaking, two cuts, which, however, we shall not look upon as essentially different; this cut possesses, besides, the property that either among the numbers of the first class there exists a greatest or among the numbers of the second class a least number and conversely, if a cut possesses this property, then it is produced by this greatest or least rational number.[2]

There are many cuts which are not produced by rational numbers. For example, (A_1, A_2) is not formed by a rational when A_1 is the set of all rational numbers whose square is less than 2 and A_2 consists of those rational numbers not in A_1. Since the square root of 2 is not rational, A_1 does not have a greatest member and A_2 does not have a least member.

The next step is very important as Dedekind takes us from the existence of cuts which are not formed by rational numbers to the existence of irrational numbers.

> Whenever, then, we have to do with a cut (A_1, A_2) produced by no rational number, we create a new, an irrational number α, which we regard as completely defined by this cut (A_1, A_2); we shall say that the number α corresponds to this cut, or that it produces this cut. From now on, therefore, to every definite cut there corresponds a definite rational or irrational number, and we regard two numbers as different or unequal always and only when they correspond to essentially different cuts.[3]

This reduces all mathematical operations involving the rationals and irrationals to operations dealing with cuts of the rational numbers. We have seen that a rational number corresponds to the cut which it produces. The irrationals are now created by means of cuts which are not formed by rationals. Since the irrationals are completely defined by these cuts, they may be identified with these cuts which are not formed by rational numbers. The basic relation between the reals is the relation $>$ It is defined in terms of the relation $>$ between cuts. We shall now show when the relation $>$ occurs between two cuts. Let (A_1, A_2) and (B_1, B_2) be any two cuts. $(A_1, A_2) > (B_1, B_2)$ if and only if there are at least two members of A_1 which are not members of B_1. The set of real numbers, R, are defined as the set of numbers which correspond to a cut of the rationals. Let $\alpha, \beta \in R$ be such that α corresponds to (A_1, A_2) and β corresponds to (B_1, B_2), then $\alpha > \beta$ if and only if $(A_1, A_2) > (B_1, B_2)$.

The preliminary groundwork is now laid for Dedekind's theorem which proves that the reals are continuous. Since this theorem relates directly to Wittgenstein's criticisms, the proof of the theorem will be provided.

If R is separated into two non-empty disjoint classes $\mathfrak{a}_1, \mathfrak{a}_2$ such that every member of $\mathfrak{a}_1$ is less than every member of $\mathfrak{a}_2$, then there is exactly one real number which executes this separation.[4]

Pf: This separation of R also establishes a cut of rationals. If A_1 is defined as the set of rationals that are members of $\mathfrak{a}_1$ and if A_2 is defined as the set of rationals that are members of $\mathfrak{a}_2$, then (A_1, A_2) is a cut of the rationals. Let α be the number which corresponds to this cut.

If $\beta \neq \alpha$, there are infinitely many rationals between α and β. Let c be one of these.

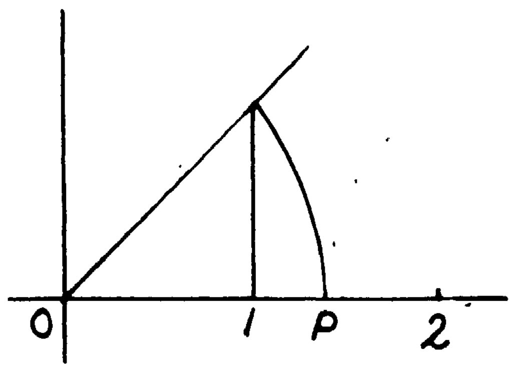
Case 1: $\beta < \alpha$, thus $c < \alpha$ and, as a result of this, $c \in A_1$. By definition of A_1, we have $c \in \mathcal{a}_1$. Since $\beta < c$, $\beta \in \mathcal{a}_1$.

Case 2: $\beta > \alpha$. Thus $c > \alpha$ and $c \in A_2$. Hence $c \in \mathcal{a}_2$ and since $\beta > c$, therefore $\beta \in \mathcal{a}_2$.

The conclusion of these facts is that every number β, $\beta \neq \alpha$, is a member of $\mathcal{a}_1$, or $\mathcal{a}_2$, depending on whether $\beta < \alpha$ or $\beta > \alpha$. As a result of this, α is either the greatest member of $\mathcal{a}_1$ or the least member of $\mathcal{a}_2$. Therefore there is exactly one cut α which executes the separation of R into $\mathcal{a}_1$ and $\mathcal{a}_2$. This completes the proof.[5]

(B) Reals and the Number Line

Wittgenstein was not satisfied with Dedekind's introduction of the irrationals into his number system. He believed that this system was based on overlooking the fundamental difference between the rationals and the irrationals. It was also Wittgenstein's contention that Dedekind's construction of the irrationals employs a picture which introduces confusion about the nature of the real numbers. These themes are found in the following remark which we quote in full.

> The misleading thing about Dedekind's conception is the idea that the real numbers are there spread out in the number line. They may be known or not; that does not matter. And in this way all that one needs to do is to cut or divide into classes, and one has dealt with them all.
>
> It is by combining calculation and construction that one gets the idea that there must be a point left out on the straight line, namely P,
>
> 
>
>
> if one does not admit 2 as a measure of distance from 0. 'For, if I were to construct really accurately, then the circle would have to cut the straight line between its points.'
>
> This is a frightfully confusing picture.
>
> The irrational numbers are - so to speak - special cases.
>
> What is the application of the concept of a straight line in which a point is missing?! The application must be 'common or garden'. The expression "straight line with a point missing" is a fearfully misleading picture. The yawning gulf between illustration and application (<u>Remarks</u>, p.151,no.37).

In the first paragraph of this passage Wittgenstein voices his objection to Dedekind's view that the real numbers are spread throughout the number line. This objection is based on the argument which he provides in the subsequent paragraphs.

Wittgenstein emphasizes the fact that it is the combination of calculation and construction which results in the idea that a point is left out of the line if $\sqrt{2}$ is not accepted as a measure of distance. He is correct on this point as there are two steps in the demonstration that a point is left out. The first step is to show that the hypotenuse of the triangle is of length $\sqrt{2}$. This requires a simple calculation, namely $\sqrt{1^2+1^2} = \sqrt{2}$. The second step is the demonstration that OP and the hypotenuse have the same length. This results from the fact that OP was constructed by means of a compass whose endpoints were originally applied to the hypotenuse.

Wittgenstein wants to show that there is nothing in the line segment OP itself that would indicate that its length was not rational. That is to say it is impossible to observe a gap in the rationals that would be filled by the point P.[6] Wittgenstein is thus proposing a difference between the rationals and the irrationals with respect to the number line. The rationals are numbers which naturally belong to the picture of the number line whereas the irrationals are derived numbers which are associated with the number line by means of calculation and construction. It is this aspect of the irrationals which makes them "special cases" according to Wittgenstein.

In a similar vein, Wittgenstein does not think that the Dedekind cut is an actual cut of the number line:

> In Dedekind we do not make a cut by cutting, i.e. pointing to the place, but - as in finding $\sqrt{2}$ - by approaching the adjacent ends of the upper and the lower class (Remarks, p.150, no.34).

Wittgenstein, in saying that we do not point to the place where the Dedekind cut is performed, puts forward the opinion that there is no place on the line which separates the line in the manner of a Dedekind cut. The cut does not have a place of its own on the line rather it is defined in terms of rationals which do have their own place on the number line.

With these observations in mind, I think it is easy to see why Wittgenstein was against the pictures associated with the concept of a cut between two points and the concept of a line in which a point is missing. I am not saying that Wittgenstein was justified in his opposition to these pictures. My intention at this time is simply to show that Wittgenstein's opposition to these pictures is a consequence of his attitude described in the previous paragraphs, towards real numbers and Dedekind cuts as they relate to the number line.

Wittgenstein claims that "straight line with a point missing" is a misleading picture since, under Wittgenstein's scheme of things, the missing point - $\sqrt{2}$ - is not a point of the line in the first place. Apparently he holds that only the rational points are given by the picture of a line while the constructions needed for such irrational points as $\sqrt{2}$ go beyond what is given in the picture. If this is his view, then we can understand why he stated that the picture which is intended to clarify the concept of a real number is not an application (Remarks, p.151,no.37).

For our purposes, the most important aspect of an application is its similarity to a model. In particular, the sentences of the theory are true in both the model and the application of the theory. We may generally take a theory to be a set of statements but we are concerned, in particular, with Dedekind's theory of the real numbers. Let us take an illustration of a theory (or some part of a theory) to be any picture which is intended to be a clarification of the theory (or part of it). Normally we use illustrations to give someone an idea of what it is that we are talking about in our theory. The illustration, if it is to be successful, should be easier to grasp than the theory which it illustrates. If we have a picture or illustration which is not an application there will be some relevant aspect of the picture which does not correlate with the theory in question. In other words, there will be a statement that is true of the picture which is not true in the theory. Whenever this occurs it is obvious that the illustration does not provide a clarification of points where the picture does not correlate. On the contrary, it gives one an incorrect impression of the theory.

This supports Wittgenstein's remark that:

> Only in so far as the illustrations are also applications do they avoid producing that special feeling of dizziness which the

> illustration produces in the moment at which it ceases to be a possible application; when, that is, it becomes stupid (Remarks, p.148,no.29).

In the particular case of Dedekind's theorem the picture will produce dizziness if the irrationals in Dedekind's system do not behave as proper points on the number line. We may now proceed to determine whether Wittgenstein's criticism of Dedekind's treatment of the reals is justified.

I should like to demonstrate that Wittgenstein had a preconceived notion of the number line and that he interpreted the picture of (Remarks, p.151,no.37) by means of this preconceived notion. In this manner all that Wittgenstein has shown is that Dedekind's theory does not match with Wittgenstein's interpretation of the picture. He has not shown that the picture in question is incompatible with Dedekind's theory. In point of fact, as we shall presently see, this picture is given a reasonable interpretation within Dedekind's theory.

When Wittgenstein claimed in (Remarks, p.151,no.37; p.150,no.34) that it was a combination of calculation and construction which indicated that $\sqrt{2}$ was left out of the line and that we do not point to the place where the Dedekind cut is made,[7] he tacitly assumed that the rationals from which the irrationals are constructed are points on the number line pictured in (Remarks, p.151,no.37). This interpretation which Wittgenstein gives to the picture is not fair to Dedekind for it overlooks the fact that in Dedekind's theory both the rationals and irrationals are cuts.

Dedekind built his theory of real numbers on the base provided by the rational numbers. Let us call these ordinary rationals, which existed before Dedekind's theory, "preliminary rationals". Both the rationals and irrationals of Dedekind's theory are cuts of the preliminary rationals. To claim as Wittgenstein did that the point P in the picture of (Remarks, p.151,no.37) is a real number that is not actually a point of the line is to presuppose that the arc which produces P is a cut of the preliminary rationals since $\sqrt{2}$ is constructed by means of this arc and $\sqrt{2}$ is a cut of the preliminary rationals. Since the arc produces $\sqrt{2}$ on the number line, we may conclude that Wittgenstein operated under the belief that the preliminary rationals were a part of the number line.

The picture in (Remarks, p.151,no.37), however, admits of more than one interpretation. The number line in Dedekind's theory consists entirely of cuts of the preliminary rationals. Some of these cuts are rational cuts of the preliminary rationals while other cuts are irrational cuts of the preliminary rationals. In any event the preliminary rationals are not points of Dedekind's continuum. Similarly, the arc which produces P is a cut of the reals where the reals are cuts of the preliminary rationals. Dedekind's theorem states that this arc or any other cut of the reals does not produce a new cut of the reals. This is to say that every cut of the reals can be performed or executed by a real number.

In addition to the fact that Dedekind's theory fits very well with the picture in question we should note that the Dedekindian interpretation of the picture avoids a flaw suffered by Wittgenstein's treatment of the picture. According to Wittgenstein one requires calculation and construction to determine that the irrational point P is left out of the line. The problem here is that one would also require calculation and construction to determine that a rational point such as 2 was left out of the line. The rationals are dense, hence if any rational were not to be included in the line we could not detect its absence by any method that could not be used to detect the absence of an irrational number. For example, measurement alone cannot show that 2 is left out of the line since one can approximate 2 within any nonzero degree of error. Measurement, on the other hand, does not possess this property since any single measurement will be subject to a nonzero rational margin or error. One will have to use a calculation to demonstrate that a line segment 0,A which bisects 0,4 contains a point A that is left out if 2 was not a member of the number line.

The crucial fact employed in the preceding discussion was the density of the rationals. Since Wittgenstein's treatment of the picture in (Remarks, p.151,no.37) can lead to a number line which is devoid of some rationals we can conclude that his reasons for not putting the irrationals with the rationals on the number line were too strict and were just as applicable to dense systems as they were to continuous systems such as Dedekind's. Whereas Dedekind started with the rationals and ended up with the continuum, Wittgenstein started with the rationals and then embarked on a path that leads to something less than the rationals.

(C) Dedekind Cuts

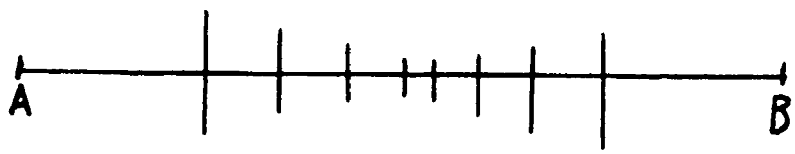
Wittgenstein has made the claim that

> Two mathematical forms, of which one but not the other can be compared in my calculus with every rational number, are not numbers in the same sense of the word.[8]

A number x is comparable, in relation to the rationals, if for every rational number q, $x < q$ or $x = q$ or $x > q$. This claim is one that is certainly acceptable since the most important relations in the system of rational numbers are those of $<$ and $=$. Wittgenstein brings this claim into play in his assessment of the theory of real numbers. In particular, he questions the result of a particular procedure which attempts to determine a definite division of the real numbers. It is this procedure to which we shall now turn.

It appears that one might divide the real numbers into two classes corresponding to a Dedekind cut by means of choosing a particular interval and then successively choosing ever decreasing nested intervals

(Remarks, p.148, no.33). One example of this procedure is to choose an interval AB and then flip a coin. If the coin turns up "heads" choose the left half of the interval and choose the right half if the coin turns up "tails". According to the same procedure one divides this smaller interval, and so on.



This picture illustrates the procedure.[9]

In (Remarks, p.149, no.33), I understand Wittgenstein to be proposing a hypothetical argument for the conclusion that this procedure produces a cut of the real numbers. This procedure, so the argument runs, results in a cut since it determines whether any number belongs to the left (lower class) or the right (upper class). Nothing in addition to the procedure is required to obtain the division of the real numbers. Any question as to the whereabouts of the point within the smallest interval that is obtained at any moment does not indicate that the procedure has failed since further applications of the procedure may provide an answer to the question. After the procedure has been carried out there is no opening for a question about the location of the place of division since every question has been answered.

The difficulty with this argument lies in the phrase "after the procedure has been carried out".

The nature of the procedure would seem to indicate that this is a procedure which has no last step and thus cannot be carried out since every application of the procedure invites a further application of the procedure. If I want to know, at any particular moment, the exact location of the point of division I am, in effect, told to wait for the end of the procedure. This is fine only if there is an end to the procedure. If there is no end to the procedure one would be better informed with the reply "There is no exact location of the point of division".

These observations do not imply that $\sqrt{2}$ is not a point of division for one does not have to wait forever, as it were, for its exact location. We must remember that $\sqrt{2}$ is not defined as 1.4142... Any definition of this sort would not yield a description that places $\sqrt{2}$ in a definite location in relation to every rational number. Wittgenstein correctly brings our attention to the fact that we have a rule for producing the decimal expansion of $\sqrt{2}$ and that it is this rule which is the number $\sqrt{2}$ (the written rule will be the numeral). He states that it is proper to speak of this rule as a number since we can calculate with this rule and compare it with other rules through the relations $<$, $=$.[10]

I believe that Wittgenstein's analysis is borne out by our calculations with real numbers. The calculation which confirms the equation " $\sqrt{2} \times \sqrt{3} = \sqrt{6}$ " does not commence with two unending, nonrepeating decimal expansions which are then multiplied together to yield a third unending, nonrepeating expansion. If we are to look on this situation in terms of decimal expansions[11] we will not have the decimal expansion of $\sqrt{6}$ before us, rather we will have, as the result of the calculation, a rule for producing the decimal places of $\sqrt{6}$.

We should note that $\sqrt{2}$ and $\sqrt{3}$ lie within a calculus that places them in a definite relation to the rational numbers and that subsequently the rules for their decimal expansions are in a definite relation to the rationals. Similarly the calculations involving these numbers result in rules for decimal expansions which are comparable to the rationals. This is not the case with the method of flipping the coin. Though one can provide a method for calculating with the procedure of flipping a coin such that the results of the calculation will be another method for deciding decimal places, the resulting method for deciding the decimal places will also involve the same flipping of the coin unless it is a trivial calculation such as multiplication by 0. We cannot even determine whether the number determined by the flipping of a coin is rational or irrational. If a pattern appears in the expansion we do not know that this pattern will continue in future throws. Similarly if no pattern appears one does not know that future throws will repeat either the initial expansion or some extension of the initial expansion. It follows from the fact that it cannot be determined whether this number is rational or irrational that it cannot be compared to every rational number.

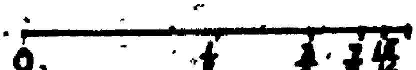
Wittgenstein concluded, in regard to the recipe for a decimal expansion through the flipping of a coin that:

> If the recipe were to correspond to a numeral at all, it would at best correspond to the indeterminate numeral "some", for all it does is to leave a number open. In a word, it corresponds to nothing except the original interval.[12]

I believe that Wittgenstein here is being as generous as possible in that it is untenable, for the reasons given in the previous paragraphs to maintain that the recipe in question determines a definite point in the interval AB. If we are to give this recipe any connectioh with the points on the interval, the best that can be said is that this recipe asks us to "pick a point, any point on AB." There is no point on AB that is picked out by the recipe to the exclusion of all other points on AB.

It is not clear to me that Wittgenstein associates a Dedekind cut with the procedure outlined in (<u>Remarks</u> p.148,no.33). In any event, we can use Wittgenstein's comments on flipping a coin to determine a point to conclude that such a procedure is in fact not a Dedekind cut. The Dedekind cut is, as we have seen in our earlier comments on Dedekind's theorem, characterized by the fact that it consists of two disjoint, non-empty, mutually exhaustive classes of rationals such that each of the rationals in one class are greater than every rational in the other. A cut of the reals can be defined in the same manner with "rational(s)" replaced by "real(s)". The procedure which Wittgenstein described does not result in a cut since we cannot say of every rational (real) whether it lies in the upper class or whether it lies in the lower class until the procedure is completed.

The difference between the procedure of flipping a coin and the rule for determining the decimal expansion of $\sqrt{2}$ essentially comes down to the degree to which these two procedures actually presuppose a completed infinite process. It might seem that they both presuppose, to the same extent, a completed infinite process since no finite decimal expansion can be the expansion of $\sqrt{2}$. This overlooks the fact that one uses the rule (for the expansion of $\sqrt{2}$) when one compares it with other real numbers. The procedure of flipping a coin can be altered in a manner that will determine a cut without presupposing the completion of an infinite process. For example, let us start with the interval $[0,1]$.



Instead of flipping a coin we are instructed first to divide the interval in half and then divide the interval to the right in half, and so on. The points which are successively chosen from the original interval are 1/2, 3/4, 7/8, 15/16, 31/32,... This process of choosing certainly can go on forever but now, unlike the case of the coin, it is not necessary for us actually to make an endless series of choices.

We can form a Dedekind cut with the new instruction. From the rule itself we can determine that the place of separation is 1 since the series $\frac{2^n-1}{2^n}$ converges to 1, without having to go through an endless process. This rule determines a cut because we can say for any real number whether it lies in the upper or lower class. We can say that this rule gives us enough information to locate the place of cutting at a definite point in relation to all of the reals. The same circumstance does not occur in the rule for flipping the coin since this rule produces a definite cut only if one has completed the endless process of flipping the coin. This leads us to the general conclusion that no Dedekind cut is produced by the completion of an endless series of operations. It is possible, however, to have a rule which determines a Dedekind cut despite the fact that the rule may describe a procedure which admits of no completion. A rule of this sort is the instruction, found in the preceding paragraph, for choosing a point from the interval $[0,1]$.

In closing I should like to make the point that the comments of Wittgenstein which we have considered in this section are helpful in clarifying the nature of a Dedekind cut. They are certainly not appropriate as criticisms of Dedekind's theory. It is not clear to me whether Wittgenstein intended them to be criticisms.

FOOTNOTES TO CHAPTER THREE

[1]Richard Dedekind; "Continuity And Irrational Numbers", p.6.

[2]*ibid*., p.13.

[3]*ibid*., p.15.

[4] α is either the greatest member of a_1, or the least member of a_2.

[5]*ibid*., p.20f.

[6]Wittgenstein makes the same point about π in *Philosophical Grammar*, p.473.

[7]We examined this on pp.65-67.

[8]Ludwig Wittgenstein; *Philosophical Grammar*, p.477.

[9]*ibid*., p.484.

[10]*loc.cit*.

[11]It is of course simpler to deal with this situation in terms of Dedekind cuts, for example, without regard to the corresponding decimal expansions.

[12]Ludwig Wittgenstein, *Philosophical Grammar*, p.485.

CHAPTER FOUR

WITTGENSTEIN CONTRA RUSSELL

Wittgenstein's criticism of Principia Mathematica as a foundation of mathematics shall be the **main topic** of investigation in this chapter. Other topics, however, will be included since they relate very closely to an understanding of Wittgenstein's remarks on Russell. We may cite, as examples of these related areas, Wittgenstein's views on mathematical proofs, axioms, notation, and consistency.

(A) Logicism

The main aim of Principia Mathematica was to establish the thesis of logicism, namely, that mathematics and logic are identical.[1] Principia Mathematica is a very practical work in relation to Russell's logicism in that it is intended to explicitly demonstrate how mathematics is logic. Wittgenstein does not believe that Principia Mathematica justifies Russell's logicism and maintains that it is built upon the mistaken view that logic is in some sense a foundation of mathematics.

Wittgenstein questions the possibility of a Russellian proof being a proof of an arithmetical statement when the latter statement is sufficiently complicated. Let us examine, with Wittgenstein, addition in Russell's system.

> But still doesn't Russell teach us one way of adding?
> Suppose we proved by Russell's method that $(\exists a \ldots g)\ (\exists a \ldots l) \supset (\exists a \ldots s)$ is a tautology; could we reduce our result to $g + l$'s being s? Now this

> presupposes that I can take the three bits of the alphabet as representatives of the proof. But does Russell's proof shew this? After all I could obviously also have carried out Russell's proof with groups of signs in the brackets whose sequence made no characteristic impression on me, so that it would not have been possible to represent the group of signs between brackets by its last term (Remarks p.66,no.4).

When the number of variables is quite small Russell's system teaches us how to add for we can easily keep track of them (Remarks p.67,no.4). Whenever the number of variables are such that they cannot be grasped when we examine them and they are not placed in a discernible sequence it does not make sense to speak of this as being addition. There is nothing inherent in Russell's system which ensures that the variables are written in a manner that will make an impression on us. This results in the interesting fact that Russellian addition may not match with our conventional addition.

If someone set before us a proof in Russell's system that $10^{10}+1=10^{10}$ we have no serious grounds within Russell's system for not accepting the proof. We would require another system to show that the proof in Principia Mathematica was suspect (Remarks p.71,no.13). Wittgenstein is not at all impressed with the ability of the logical calculus to compel in matters of arithmetic.

> It is not logic - I should like to say - that compels me to accept a proposition of the form (∃) (∃) ⊃ (∃), when there are

> a million variables in the first two pairs of brackets and two million in the third. I want to say: logic would not compel me to accept any proposition at all in this case (Remarks p.72f.,no.16).

Wittgenstein observes that logic must be supported by another calculus if it is to have the certainty that is claimed for it. In the case of the proposition with millions of variables, mentioned above, it is extremely likely that conflicting results will be obtained. The explanation which states that fatigue or some other human frailty is responsible for the discrepancy is based upon an acceptance of the statement '1,000,000+1,000,000=2,000,000'. Without this type of support there is no convincing argument that a particular arithmetic conclusion to the proposition with many variables is the correct one.

> A shortened procedure tells me what ought to come out with the unshortened one. (Instead of the other way round.) (Remarks p.74,no.18).

This would indicate that a system such as Principia Mathematica obtains its strength from ordinary arithmetic and thus it cannot be the case that mathematics is logic.

Wittgenstein's description of the situation is an accurate portrayal of how we would evaluate the result of a lengthy Russellian calculation. It is arithmetic (the shortened procedure) which tells us what the outcome of a Russellian (the unshortened) calculation should be.

Wittgenstein does not rule out the possibility of proving that there is for each proof in ordinary mathematics a Russellian proof which corresponds to it. Wittgenstein's claim is that "...the acceptance of such a correspondence does not lean on logic" (Remarks p.89,no.53). Thus it is Wittgenstein's opinion that a correspondance between logic and mathematics does not in itself demonstrate that mathematics is logic. We can see from the cited remark that Wittgenstein believed that Russell's system would prove that mathematics is logic if the acceptance of the correspondence between logic and mathematics could be justified by logic alone.

This criticism of Russell's logicism is appropriate. The fact that there is a logical proof corresponding to each mathematical proof merely shows that mathematical proofs can be translated into logical proofs. It does not necessarily show that mathematics is logic.[2] In Principia Mathematica one will find logical formulas. The correspondence between one of these formulas of logic and a mathematical statement, however, will not be found in Principia Mathematica as a formula of logic. Thus, one requires something more than Principia Mathematica to establish this correspondence.

The correspondence between Russellian arithmetic and ordinary arithmetic is in fact established by induction. For example, one may demonstrate that Russellian addition agrees with normal addition in two steps. The basic step will be a proof that for every natural number m, m+0=m in Russellian arithmetic. The second step is to prove that for every natural number n if m+n in Russell's system agrees with the sum m+n in ordinary arithmetic for every natural number m, then m+(n+1) in Russellian arithmetic must agree with the sum m+(n+1) in ordinary arithmetic.

This supports Wittgenstein's contention that it is not logic that proves that there are logical formulas which correspond to arithmetical statements. The proof by induction which we have outlined above is not one that can be carried out in Principia Mathematica. The proof might best be described as a particular instance of the very general principle of mathematical induction.

Russell's system falters in comparison with ordinary arithmetic since its notation does not facilitate the calculations and proofs which are found in normal mathematics. The introduction of new symbolism is not at all a trivial matter for Wittgenstein. The techniques of dividing and adding are not taught in Russell's logic (Remarks p.89,no.52;p.69,no.8). We may define a paradigm of an arithmetic equation to be any calculation or procedure which yields that particular equation and no other equation. For example, the following calculation is a paradigm of the equation 16x17=272:

$$\begin{array}{r} 17 \\ \underline{16} \\ 102 \\ \underline{17} \\ 272 \end{array}$$

It is a paradigm in that it exemplifies what it means to multiply 17 by 16. Once this calculation is given it is not necessary to go through other procedures to ascertain the result of multiplying 17 and 16 together.

Normal mathematical calculations function very well as paradigms wheras their translations into Russell's notation are in most cases not in the least appropriate as paradigms. This is considered sufficient reason, by Wittgenstein, for not regarding the Russellian statement as a proof:

> ...something stops being a proof when it stops being a paradigm, for example Russell's logical calculus; and on the other hand any other calculus which serves as a paradigm is acceptable (Remarks p.72,no.14).

A difference in notation is thus seen as changing a proof into something which is not a proof. This relates to our remarks in Chapter One where we noticed that Wittgenstein, like Kant, grounded the universality of a proof in the general procedures which governed the construction. When a very cumbersome notation is adopted the same rules of construction will not apply to the translation of the original proof; furthermore, there might not be a translation of the first set of rules which preserves their clarity (Remarks p.70,no.11). We are referring to the perspicuity of proofs in general and, in opposition to strict finitism, are not placing restrictions on the entities to which the constituents of the proof must refer.

The sense of a proof depends upon the notation and its application. If Principia Mathematica is to relate to anything outside of itself such as ordinary mathematics, it is imperative that "V" and "~" have a familiar application (Remarks p.79, no.34). This application is not a part of the calculus since it refers to a useage in an established area. For example "V" and "~" have their applications in the words "or" and "not" in the English language.

Every system of proofs must have a starting point. In the case of Principia Mathematica the primitive propositions[3] are fundamental.

> To repeat, in a certain sense even Russell's primitive propositions convince me. Thus the conviction produced by a proof cannot simply arise from the proof-construction (Remarks p.79, no.35).

The thought here is that the acceptance of a proof involves the acceptance of both the primitive propositions of a system and the proof-construction. The proof-construction can be justified within the system by citing the earlier steps and rules which led to each line of the proof. It is at this point where the convincing power of a proof comes from an area external to the system.

Without the application which we have been considering the system would, in Wittgenstein's opinion, lose its strength. All that would remain is a collection of symbols. The symbols without the application are compared to the standard metre without its employment by people as a standard.

> If I were to see the standard metre in Paris, but were not acquainted with the institution of measuring

> and its connection with the standard metre - could I say, that I was acquainted with the concept of the standard metre? (Remarks p.80,no.36).

Wittgenstein admits to the possibility of a sequence of signs, as such, being accepted as a proof. The signs are not accepted by virtue of their having any particular pattern that makes it easy to associate them in a certain manner, nor are they accepted because of some rule which permits the second group of signs to follow the first group. Wittgenstein gives us some indication as to how this situation could arise.

> A proof could surely consist of only two steps: say one proposition '(x).fx' and one 'fa' - does the correct transition according to a rule play an important part here? (Remarks p.81,no.38).

We should make it clear that Wittgenstein is not advocating this position as his final analysis of mathematical proofs. Wittgenstein regards the view which he describes as nothing more than a bare minimum that is required of a proof. The mere fact that something may be accepted as a proof by, as it were, an arbitrary decision without any justification does not result in the consequence that every proof may be accepted in this manner.

The editors of Remarks On the Foundations Of Mathematics interpreted Wittgenstein as having the position mentioned above, thus finding a tension between this position and Wittgenstein's view that Russell's proofs are convincing solely to the extent that they possess geometrical cogency.[4] Geometrical cogency is the convincing power of a proof looked upon as a transformation of signs (Remarks p.80,no.38). This tension is not actually found in Wittgenstein's remarks. Wittgenstein did not state that the calculus of Principia Mathematica should be accepted as an ultimate principle of faith requiring nothing to support its deductions. His view in this regard was diametrically opposed to any position that would allow Russell's system to go about its business without any challenge. Principia Mathematica was designed as a foundation for mathematics. The results of this system are supposed to match the results of mathematics, therefore it is not to be reggrded as a self-subsistent entity with no connection or relationship to any other system. If this system were to be regarded as an independent one, there would be nothing wrong with some proofs which resulted in a statement such as "1,386+3,792=5,180". If the attempt were made to patch up the system by arbitrarily decreeing that all the results of Principia Mathematica must match with normal mathematics, then one would have to give up the view that the system was independent since the results of the system rely upon the results of normal mathematics.

We conclude from these considerations that Russell's system must meet some criterion if the proofs which are carried out within it are to be accepted as mathematical. Russell's proofs, as we have seen, do not support many trust in these systems as the proper supporting structures for the rest of mathematics. Wittgenstein essentially ignored the interesting aspects of the systems, restricting his efforts to ascertaining whether the foundational system can at least yield mathematical calculations. We have seen in this chapter that when a system such as Principia Mathematica does not demonstrate, entirely within its own means, that $10^{10}+1 \neq 10^{10}$, Wittgenstein considered it incorrect to equate together mathematics and the system in question.

The same approach was also of benefit in regard to the consistency problem. A contradiction, examined within the confines of a system such as Principia Mathematica, is devastating. The same contradiction, examined from the vantage point of normal mathematics, is totally harmless. The crisis in the foundation of mathematics in the early 1900's was certainly not a problem for mathematics as a whole. It seems to me that Wittgenstein's remarks on consistency are not nearly as controversial as they might appear to be. He is simply taking, for the most part, the attitude of the normal mathematician towards the paradoxes.

mathematical statements when the number of variables cannot be clearly taken in. Hence, Russell's system certainly does require the geometrical cogency of which Wittgenstein spoke. This is demanded of Russell's calculus since it attempts to show that mathematics is logic.

(B) Formalism And Consistency

Wittgenstein's comments on this topic bear upon the formalist philosophy of mathematics. In its most extreme form the formalist doctrine states that mathematics is a game with symbols which have no meaning. Wittgenstein does not accept this theory since any game with meaningless symbols would not possess the convincing power which lies within the domain of mathematics. Any cogency which the underlying logic of Principia Mathematica possessed came from the fact that the symbols "V" and "$\sim$" had some meaning or application outside of Russell's calculus.[5] The same argument may be applied to any other system of symbols.

Curry proposed a formalist definition of mathematics as the science of formal systems in general. Mathematics deals only with formal structure and not with any particular system.[6] Curry's conception of a formal system is quite strict in that it demands lists of: tokens, operations, rules of formation, elementary predicates, axioms or axiom schemata, and rules of procedure.[7] The cart is here being placed before the horse. Arithmetic certainly has calculations which transcend the capabilities of formal systems such as Principia Mathematica. Furthermore, Wittgenstein's observation that we use our ordinary arithmetic to check on the correctness of formalized arithmetic,[8] demonstrates that our elementary arithmetic is of greater cogency than formal systems. This is not to say that formal systems have no place in mathematics. There are many issues, such as the continuum hypothesis where the techniques involved in formal systems are helpful. The fact that arithmetic is not the same as logic does not lead to the conclusion that mathematics and formal systems have nothing to do with each other.

Hilbert gave elementary number theory a somewhat higher position in his philosophy of mathematics as he did not wish to employ any methods in his proof theory which went beyond finitary number theory.[9] Hilbert wanted to use the devices of number theory to support the mathematics of Cantor's paradise. Higher mathematics is built on the groundwork of finitary statements through the addition of ideal statements.[10] In this respect Hilbert avoided the criticisms of Wittgenstein against these systems which were constructed with the thought that they did not have to rely on basic arithmetic.

Hilbert's next concern was to establish the consistency of his system:

> There is just one condition, albeit
> an absolutely necessary one,

> connected with the method of ideal elements. That condition is a proof of consistency, for the extension of a domain by the addition of ideal elements is legitimate only if the extension does not cause contradictions to appear in the old, narrower domain, or, in other words, only if the relations that obtain among the old structures when the ideal structures are deleted are always valid in the old domain.[11]

There is an additional blessing in all of this for Hilbert - the consistency of the axioms of arithmetic are finally proved.[12]

Wittgenstein, as we shall soon discover, did not share in Hilbert's enthusiasm for consistency. Arithmetic is placed on such a high level by Wittgenstein that the discovery of a contradiction in arithmetic would demonstrate that we had an improper notion of certainty rather than provide grounds for considering our arithmetic as being unsatisfactory (<u>Remarks</u> p.181,no.28). We do not have to take Russell's paradox very seriously. The alternative which Wittgenstein proposed is quite convincing. Contradictions may be treated with the same action as the discovery of division by 0 received.

> Let us suppose that people originally practised the four kinds of calculation in the usual way. Then they began to calculate with bracketed expressions, including ones of the form (a-a). Then they noticed that multiplications, for example, were becoming ambiguous. Would this have to throw them into confusion? Would they have to say: "Now the solid ground of arithmetic seems to wobble"?
>
> And if they now demand a proof of consistency, because otherwise they

> would ~~be in~~ danger of falling into the bog at every step - what are they demanding? Well, they are demanding a kind of order. But was there no order before? - Well, they are asking for an order which appeases them now. - But are they like small children, that merely have to be lulled asleep? (Remarks p.100f., no.78).

Wittgenstein did not approve of this desire for order as he thought it was unreasonable. He was not opposed to consistency proofs per se (Remarks p.106, no.82). The harm comes from those who look towards a consistency proof for a justification of the calculus which makes it unassailable from any angle. The directions from which the paradoxes arise are irregular; there was no intention to stop these paradoxes in the original calculus.

If we use the signs of the calculus without any thought as to their application one may employ the expression "f(f)" which leads to a paradox, however if one pays attention to the application of the signs one will not think of writing "f(f)" (Remarks p.105,no.81). Wittgenstein is drawing attention to the fact that such an anomalous situation will not arise within any normal arithmetic context. He makes this point in a rather extreme fashion when he states:

> I should like to ask something like: "Is it usefulness you are out for in your calculus? - In that case you do not get any contradiction. And if you aren't out for usefulness - then it

It is important to note that Wittgenstein confined his comments to truth and provability as they relate to Russell's system. If Wittgenstein were to have made a blanket claim such as truth is provability, then we would have evidence that he rejected studies which involve completeness. Certainly for any system such as Russell's there are statements which are true but not provable if the system is consistent. Wittgenstein's comments on Gödel's theorem do not indicate that he was guilty of denying this claim. His remarks were based on the distinction between truth as it could be expressed by means of Russell's system and truth as it could be expressed in other systems.

We have been very sympathetic towards Wittgenstein's appraisal of Gödel's theorem as those comments of his which we have studied up to this point were justified by the methods found in Gödel's proof. The situation will be balanced when we test the accuracy of Wittgenstein's other views on Gödel's theorem. These views indicate a lack of understanding on Wittgenstein's part.

We agree with Anderson's observation that Wittgenstein ignored the assumption in Gödel's proof that Principia Mathematica was consistent.[13] Anderson refers to Wittgenstein's restatement of Gödel's proof in (Remarks p.50,no.8). The same oversight appears in (Remarks p.51,no.11; p.53,no.17) where, within the context of his discussion of Gödel's theorem, Wittgenstein declares that

> a proposition yields its contradictory, and vice versa? - the proposition itself is unusable, and these inferences equally; but why should they not be made? - It is a profitless performance! - It is a language - game with some similarity to the game of thumb-catching (Remarks p.51,no.12).

Alan Ross Anderson thought that Wittgenstein was excessive in his devaluation of consistency proofs. His argument, in essence, states that we may look upon the foundations of mathematics as a language-game where it is very important to avoid contradictions.[13]

We agree with Anderson to the extent that he recommends that we use language-games to analyze the role of formal systems in mathematics[14] but his other comments on consistency are misplaced. He thought that Wittgenstein might be asking us to forsake the playing of the consistency-game. In support of this thought Anderson quotes the following passage

> 'But didn't the contradiction make Frege's logic useless for giving a foundation to arithmetic?' Yes, it did. But then, who said that it had to be useful for this purpose? (Remarks p.171,no.13).

Anderson's response to this remark is that Frege, Peano, Russell, Whitehead, and Hilbert thought that Frege's logic had to be useful for giving a foundation to arithmetic.[15]

I believe that Wittgenstein's criticism of Frege is justified. Frege's aim was to provide a logical calculus which would secure once and for all the certainty of mathematics.[16] Wittgenstein's rhetorical question concerning Frege's logic, referred to by Anderson, is an attack upon Frege's desire to certify arithmetic. If Frege had simply looked upon his logical calculus as a new mathematical subject, he would not have met with Wittgenstein's criticism, for this position is in accord with the opinion expressed in (Remarks p.167,no.9). Frege's ambition was far greater than this. Anderson did not take Wittgenstein's remark as an assault on the very idea of a foundation for mathematics. Logic simply cannot accomplish everything that ordinary arithmetic can achieve. Frege and those who followed him were guilty of that quest for a certainty above and beyond ordinary arithmetic. Their work is an extremely interesting part of mathematical logic but, for the reasons which we have studied earlier, not a foundation for mathematics.

Russell was closer to the mark in his understanding of logicism and contradictions. He maintained that logicism attempts to show that the theorems of arithmetic are derivable from the premises of logic. Russell admits that the latter are not nearly as obvious as the former but this is not upsetting since it is not part of his logicism to prove that 2+2=4.[17] On the contrary, Russell believed that arithmetical truths supplied inductive evidence for the logical premises in his system.

> Thus in mathematics, except in the earliest parts, the propositions from which a given proposition is deduced generally give the reason why we believe the given proposition. But in dealing with the principles of mathematics, this relation is reversed. Our propositions are too simple to be easy, and thus their consequences are generally easier than they are. Hence we tend to believe the premises because we can see that their consequences are true, instead of believing the consequences because we know the premises to be true. But the inferring of premises from consequences is the essence of induction; thus the method in investigating the principles of mathematics is really an inductive method, and is substantially the same as the method of discovering general laws in any other science.[18]

Russell's views are very sensible in that he does not force an alien certainty upon arithmetic. The discovery of a contradiction in the logical system gives evidence against the premises of the system[19] but does not pose any problem for normal mathematics.

One of the strongest points of Wittgenstein's evaluation of attempts at a foundation for mathematics is his emphasis upon comparing the features of ordinary mathematics with those of the foundational systems. This is a very useful method of comprehending the situation since it is extremely easy to be misled if one's focus is directed towards the foundational system by itself. The structure and results of these systems are quite fascinating and well-organized. The tendency is thus induced in us to place an unwarranted amount of trust in these systems as the proper supporting structures for the rest of mathematics. Wittgenstein essentially ignored the interesting aspects of the systems, restricting his efforts to ascertaining whether the foundational system can at least yield mathematical calculations. We have seen in this chapter that when a system such as Principia Mathematica does not demonstrate, entirely within its own means, that $10^{10}+1 \neq 10^{10}$, Wittgenstein considered it incorrect to equate together mathematics and the system in question.

The same approach was also of benefit in regard to the consistency problem. A contradiction, examined within the confines of a system such as <u>Principia Mathematica</u>, is devastating. The same contradiction, examined from the vantage point of normal mathematics, is totally harmless. The crisis in the foundation of mathematics in the early 1900's was certainly not a problem for mathematics as a whole. It seems to me that Wittgenstein's remarks on consistency are not nearly as controversial as they might appear to be. He is simply taking, for the most part, the attitude of the normal mathematician towards the paradoxes.

FOOTNOTES TO CHAPTER FOUR

[1] Bertrand Russell, The Principles Of Mathematics, v.

[2] The issue here is essentially that of reductions: The reduction of A to B requires that B is more fundamental than A. Wittgenstein argues that Russellian arithmetic is not more fundamental than ordinary arithmetic.

[3] A.N. Whitehead and Bertrand Russell, Principia Mathematica, I, 94-97.

[4] Ludwig Wittgenstein, Remarks On the Foundation Of Mathematics, p.83n.

[5] see p.87f.

[6] Haskell Curry, Outlines Of A Formalist Philosophy Of Mathematics, p.56.

[7] ibid., p.11ff.

[8] see p.84.

[9] David Hilbert, "On The Infinite", p.142f.

[10] ibid., p.145.

[11] ibid., p.149.

[12] ibid., p.149f.

[13] Alan Anderson, "Mathematics And The 'Language Game,'" p.488.

[14] This topic will be dealt with in Chapter Seven.

[15] Alan Anderson, op.cit., p.489.

[16] Gottlob Frege, The Foundations of Arithmetic, IX.

[17] Bertrand Russell, "The Regressive Method of Discovering the Premises of Mathematics,"

[18] ibid., p.273f.

[19] ibid., p.279f.

CHAPTER FIVE

TRUTH, PROVABILITY, AND GÖDEL'S THEOREM

(A) Wittgenstein On Gödel's Theorem

Wittgenstein's remarks on Gödel's first incompleteness theorem have definitely not been received with great joy. This is understandable since some of these remarks are rather misdirected as they do not have anything to do with Gödel's theorem. On the other hand, Wittgenstein's remarks on truth and provability can be supported by Gödel's first incompleteness theorem.

Wittgenstein argues against interpreting Gödel's result as demonstrating that there are true propositions in Russell's system which are not provable in his system (Remarks p.50,no.5). A part of Wittgenstein's motivation for holding this view is evident in the following remark which we quote in full.

> For what does a proposition's 'being true' mean? 'p' is true = p. (That is the answer.)
> So we want to ask something like: under what circumstances do we assert a proposition? Or: how is the assertion of the proposition used in the language-game? And the 'assertion of the proposition' is here contrasted with the utterance of the sentence e.g. as practice in elocution, - or as part of another proposition, and so on.
> If, then, we ask in this sense: "Under what circumstances is a proposition asserted in Russell's game?" the answer is: at the end of one of his proofs, or as a 'fundamental law' (Pp.). There is no other way in this system of employing asserted propositions in Russell's symbolism" (Remarks p.50,no.6).

Wittgenstein is using the Tarski "quotation-mark name" criterion of truth[1] to proceed from the truth of a statement to the assertion of a proposition. This association of truth and assertion was also accepted by Russell. The assertion of a proposition in a system must be separated from other propositions which can be formed within the system. The statement "$p.\sim p$" is an example of a well-formed formula of Russell's system but this does not necessarily imply that Russell's system commits itself to this statement. Another illustration of this separation is found in the statement "$:p.\sim p.v.\sim(p.\sim p)$". This statement is supported or backed up by Principia Mathematica though the constituent statement "$p.\sim p$" is not, we trust, supported by the system. Russell, following Frege, employed "$\vdash$" as the symbol for assertion and translated it as "it is true that".[2]

The next step undertaken by Wittgenstein is the connection of "$\vdash$" with the proofs of Principia Mathematica. This is very easy to accomplish since the only statements of the system which are not deduced from other statements and which are not constituents of other statements are the primitive propositions (Pp.) of the form "$\vdash$ ...". That is to say, the only one line proofs in Principia Mathematica are primitive propositions which are preceded by the sign "$\vdash$".[3] The converse of this, namely that every primitive proposition of the form "$\vdash$ ..." is a one line proof, is also true. This is the basic step for an inductive proof that "$\vdash$" is used as a prefix to all and only those statements which occur at the end of a proof.

This substantiates Wittgenstein's claim that truth and provability are the same in Russell's system. Anderson has not accepted this claim since it does not do justice to Gödel's theorem and is inconsistent with Wittgenstein's declarations that he has nothing against the results of Gödel's theorem. "If the proof does not show that truth outruns provability in PM (provided the system is consistent), then what of importance does it show?"[4]

Anderson relies on the fact that provability is a syntactic property whereas truth is a semantic property. Wittgenstein, it appears, rejects those areas of mathematical logic which involve completeness and does not properly use the phrase "true in Russell's system".[5]

I think it is safe to say that Wittgenstein was not familiar with the concepts of completeness and semantics. On the other hand I do not find anything in Wittgenstein's comments on the connection between "true in Russell's system" and "provable in Russell's system" which are opposed to areas of mathematical logic which involve completeness.

The conflict between Anderson and Wittgenstein is based upon a disagreement as to the system in which it is determined that truth and provability do not coincide in Principia Mathematica. Wittgenstein is very strong in his conviction that such a determination will be associated with a different sense of "truth" than is found in Russell's system.

> The question is quite analogous to: Can there be true propositions in the language of Euclid, which are not provable in his system, but are true? - Why there are even propositions which are provable in Euclid's system, but are false in another system. May not triangles be - in another system - similar (very similar) which do not have equal angles? - "But that's just a joke! For in that case they are not 'similar' to one another in the same sense!" - Of course not; and a proposition which cannot be proved in Russell's system is "true" or "false" in a different sense from a proposition of Principia Mathematica (Remarks p.50,no.7).

This problem cannot be resolved without an inspection of Gödel's theorem. The basic task in Gödel's theorem is the translation of metamathematical statements into propositions of Principia Mathematica which are about natural numbers. Within the system of Principia Mathematica Gödel was able to provide a definition of "x is a provable formula."[6] This permits one to define "x is undecidable" in the system as well.[7] These definitions established the structure which culminated in the theorem that there are undecidable arithmetic propositions in Principia Mathematica if Principia Mathematica is ω-consistent.[8] Gödel proved this theorem for other systems in addition to Principia Mathematica but we shall concern ourselves with this particular system alone. We should also note that the scope of Gödel's theorem may be enlarged to include consistent systems of the sort mentioned in the theorem rather than restricting these systems to ω-consistency.[9]

Gödel's proof yields the result that if Principia Mathematica is consistent, then there is an undecidable arithmetic proposition in that system. The proof itself, without transgressing the bounds of Russell's system, does not mention any semantic property such as "truth". Gödel dealt with the truth of the undecidable proposition in his introduction to the proof.

> From the remark that [R(q);q] says about itself that it is not provable it follows at once that [R(q);q] is true, for [R(q);q] is indeed unprovable (being undecidable). Thus, the proposition that is undecidable in the system PM still was decided by metamathematical considerations.[10]

This illustrates the fact that the metamathematical notion of truth used by Gödel in the preceding paragraph was not a part of Russell's system. There was no translation of the statement "x is true" into Principia Mathematica that captured Gödel's intention, though there was a translation of "x is provable". Kreisel focused in on that apparatus which was required, in addition to Principia Mathematica, to determine the truth of the undecidable statement.

> The beauty of Gödel's own formulation is that his result can be separated from the question of truth in arithmetic. He has found a formula (x)A(x), A primitive recursive, which is formally undecidable in the given system $\mathcal{L}$ if $\mathcal{L}$ is consistent. Now, given this syntactic result one argues: since any closed proposition is true or false, either (x)A(x) or $\sim$ (x)A(x) is true, and so there is a true proposition which cannot be proved in $\mathcal{L}$ (on the intended interpretation of $\mathcal{L}$). One sees that

> all one needs of the concept of truth
> is that either R or $\sim R$.[11]

This confirms Wittgenstein's remark stating that when we speak of the unprovable proposition as being true we are employing a concept of truth which is a part of a system which is slightly larger than Russell's system. This larger system consists of Russell's system plus an axiom which states that for any statement either it or its negation is true. We must disagree with Anderson's evaluation of Gödel's theorem. Truth in the enlarged system outruns provability in Russell's system. What Gödel's theorem does in fact show is that if Principia Mathematica is consistent, then there are undecidable propositions within it. Wittgenstein's rejection of any concept of truth in Russell's system which is not provable in the system does not permit us to infer, with Anderson, that Wittgenstein seems to reject discussion of such issues as the completeness of a formal system.[12] Wittgenstein's efforts were spent in attempting to establish the fact that the considerations involving completeness are not carried out within Russell's system but find their home in a different calculus. These other systems do not challenge Wittgenstein's claim that truth and provability are the same in Russell's system since the truth of the undecidable statement (in Russell's system) is proven in the somewhat larger system in the manner described by Kreisel.

It is important to note that Wittgenstein confined his comments to truth and provability as they relate to Russell's system. If Wittgenstein were to have made a blanket claim such as truth is provability, then we would have evidence that he rejected studies which involve completeness. Certainly for any system such as Russell's there are statements which are true but not provable if the system is consistent. Wittgenstein's comments on Gödel's theorem do not indicate that he was guilty of denying this claim. His remarks were based on the distinction between truth as it could be expressed by means of Russell's system and truth as it could be expressed in other systems.

We have been very sympathetic towards Wittgenstein's appraisal of Gödel's theorem as those comments of his which we have studied up to this point were justified by the methods found in Gödel's proof. The situation will be balanced when we test the accuracy of Wittgenstein's other views on Gödel's theorem. These views indicate a lack of understanding on Wittgenstein's part.

We agree with Anderson's observation that Wittgenstein ignored the assumption in Gödel's proof that <u>Principia Mathematica</u> was consistent.[13] Anderson refers to Wittgenstein's restatement of Gödel's proof in (<u>Remarks</u> p.50,no.8). The same oversight appears in (<u>Remarks</u> p.51,no.11; p.53,no.17) where, within the context of his discussion of Gödel's theorem, Wittgenstein declares that a contradiction is without any harm. Despite the fact that he was correct in his opinion that a contradiction such as Russell's paradox posed no problem for normal mathematical work,[14] Wittgenstein was in error when he brought this point up against Gödel's work.

Gödel was not out to show that a consistency proof was essential to any proper work in mathematics. He simply used the consistency of Principia Mathematica as a premise in his proof with complete neutrality.

> Even if an inconsistency didn't 'matter', one cannot hope to discuss significantly on this basis a result which explicitly supposes consistency of the system.[15]

A proper analysis of Gödel's incompleteness theorem must investigate what Gödel achieved under the hypothesis of consistency. The harmfulness of contradictions has nothing to do with Gödel's proof.

Wittgenstein's comments on the interpretation which the Gödelian statement is to receive demonstrate that Wittgenstein did not understand the manner in which the interpretations were assigned. He has given definite priority to a direct proof of the Gödelian statement over the interpretation of it. The interpretation can easily be changed, Wittgenstein proposed, to accommodate the proof. A proof of the Gödelian statement may stand if we withdraw the interpretation that this statement be translated as "the Gödelian statement is not provable" (Remarks p.51,no.10). Wittgenstein applies the same reasoning to a proof of the negation of the Gödelian statement; the question of this being a proof of the Gödelian statement is again a matter of interpretation (Remarks p.53, no.17).

Wittgenstein is not correct in the distinction which he is making between a proof of Gödel's statement and the interpretation which is given it. Gödel translated all of the important notions relative to Russell's system. By this I mean that "x is a provable formula" was taken as "x is a Russell-provable formula." It is very easy to check Gödel's method of translating metamathematical notions into propositions about natural numbers. His definitions captured the meanings of such terms as "variables", "formulas", etc. This was very ingenious on Gödel's part since it essentially allows us to discuss Russell's system within his own system.

Wittgenstein's movements are not sufficient to permit the Gödelian statement to flee from the bounds which Gödel has erected around it. The Gödelian statement is a proposition about natural numbers. If it is proved within Russell's system we may assert that a certain property belongs to each of the natural numbers. One does not have to go further than this very proof to discover a Russellian proof of the negation of the Gödelian statement. We are thus given as much evidence, within Russell's system, for the negation of the Gödelian statement as we have evidence for its affirmation, assuming that the latter is proved in Russell's system.

Wittgenstein did not grasp the cleverness of the devices that Gödel implemented. In Gödel's proof we do not basically have a statement which translates under some interpretation, into its negation. Wittgenstein thought that a proof of the Gödelian statement would leave us with one proof and one interpretation. This facilitated Wittgenstein's dismissal of the negation of the Gödelian statement, given that its affirmation was proved. Since it is not proved but only interpreted it does not stand with the firm footing of a proved proposition. What Wittgenstein faced in actuality were two proofs of two contradictory statements.

(B) Meaning As Use

We will now take a look at Gödel's theorem from a totally different angle. Dummett has examined Gödel's theorem with respect to Wittgenstein's doctrine of meaning as use. In the first part of "The Philosophical Significance of Gödel's Theorem" Dummett sets forth an argument to the effect that the result of the first incompleteness theorem demonstrates that meaning cannot be reduced to useage. Dummett then explains why this argument fails to achieve its purpose.

The first argument, against the meaning as use theory, gives the following analysis of the Gödelian statement U:

> Since U is neither provable nor refutable, there must be some models of the system in which it is true and others in which it is false. Since,

> therefore, U is not true in all models of the system, it follows that when we say that we can recognise U as true we must mean 'true in the intended model of the system! We thus must have a quite definite idea of the kind of mathematical structure to which we intend to refer when we speak of the natural numbers and it is by reference to this intuitive conception that we recognise the statement U to be true. On the other hand, we can never succeed in completely characterising this intuitive conception by means of any formal system, that is, by any finitely stateable stipulation of the set of statements about natural numbers which we are prepared to assert.[16]

On the basis of this apprehension of Gödel's theorem it is proposed that any account of the natural numbers which attempts to make clear the fact that every model in which U is false is a model which has things which are not our intuitive natural numbers will use words of the kind "set" and "finite". If we were to elucidate the meanings of these words by declaring which statements are to be made about them, this elucidation would correspond to a formal system that would be sufficiently sophisticated to define the natural numbers and would therefore be subject to Gödel's theorem. In this system there will be another Gödelian statement which cannot be proved in the system but which we view as being intuitively true. Thus, for any account of the use of "natural number" there will be a true arithmetical statement that is not in the given account. Meaning cannot therefore be reduced to useage.[17]

We pay a rather high price for rejecting this doctrine. Those who reject the doctrine replace it with the belief that concepts are in the mind and that training in the use of a word brings the concept to life and teaches us to pair the word and concept together; however, as the preceding argument indicates, the concept surpasses any finite description of the use of the word. This is a problem for any theory of meaning because we will not have any reliable method for identifying one person's understanding of a word's meaning with that of another person's understanding. This distance between minds renders the entire notion of meaning as superfluous. Under this scheme, the presence or absence of meaning has no effect.[18]

Dummett's last point is an accurate summary of Wittgenstein's opposition towards meaning as an entity that resides in the mind. An example of Wittgenstein's application of the meaning as use doctrine, with the corresponding rejection of meaning as a mental attitude, is found in some of his remarks on axioms. An axiom is not an axiom simply as a result of it being self-evident. The fact that a certain proposition is accepted constitutes the main ground for viewing it as an axiom (Remarks p.113,no.2). In answer to the question "But as what do we accept it?" (Remarks p.113,no.2), Wittgenstein states that it is the special use that is given to the proposition which shows its meaning:

> I want to say: when the words of e.g. the parallel-axiom are given (and we understand the language) the kind of use this proposition has and hence

> its sense are as yet quite undetermined. And when we say that it is evident, this means that we have already chosen a definite kind of employment for the proposition without realizing it. The proposition is not a mathematical axiom if we do not employ it precisely for this purpose (<u>Remarks</u> p.113,no.3).

Wittgenstein, in the same remark, provides an indication of the use to which an axiom is employed. One does not confirm or attempt to prove an axiom by means of an experiment. Generally speaking, the assertion of a proposition without evidence is considered to be a mark of naivete and lacking any scientific rigor. An axiom is the exception to this rule. It may be asserted without support. This brings to light that it is not any mental act that distinguishes an axiom from other propositions since there are many other propositions which are as self-evident as an axiom but require confirmation in the particular system wherein they are stated.

> One might, so to speak, preface axioms with a special assertion sign.
> Something is an axiom, not because we accept it as extremely probable, nay certain, but because we assign it a particular function, and one that conflicts with that of an empirical proposition.
> We give an axiom a different kind of acknowledgment from an empirical proposition. And by this I do not mean that the 'mental act of acknowledgment' is a different one.
> An axiom, I should like to say, is a different part of speech (<u>Remarks</u> p.114,no.5).

Although Wittgenstein contrasts axioms with empirical propositions, his comments apply to axioms in any system. His statement that an axiom is a different acknowledgment than other propositions fits in very well with the notion of an axiom as it is found in formal systems. The extent to which one has a conviction about the truth of an axiom is of no consequence to the system. The important characteristic of an axiom is its ability to be stated at any time in any proof without derivation from any previous statement. We may conclude from this that the meaning of an axiom was, for Wittgenstein, found in its use.

Despite the fact that Dummett discarded theories of meaning which do not equate meaning with use, he is very careful to make it clear that there are limits to what can be accomplished by this doctrine. There is reason to believe that there is no complete characterisation of the application of the word "funny".[19] For any account of "funny" we can probably come up with an exception to the account. We should therefore not expect complete precision from the meaning as use doctrine,[20] nevertheless it is the only way of avoiding a theory that makes meaning an inaccessible concept.

Dummett, with the hope of preserving the meaning as use doctrine, uncovers the error in that interpretation, given earlier of Gödel's theorem as an example of meaning that cannot be reduced to use. He correctly argues that in the account of Gödel's theorem which we are investigating there is an equivocation on the term "model". When the intended model which we intuitively grasp is described in the argument it is assumed that this model is a model in the same sense as the mathematical models of Principia Mathematica. The intended model is not a mathematical model as the latter concept is given a very precise formulation in mathematics while the former notion is not subject to the same rigorous standards of formulation.[21] A mathematical model of a language demands a well-defined domain and an interpretative function which associates symbols of the language with members, functions, and relations of the domain. Since the intuitive model is not given to us by means of such specifications, it is not properly speaking a mathematical model.

We are now in a position to see where the original argument against the meaning as use doctrine failed. This argument maintained that there was a meaning to the expression "natural number", captured by the intuitive model, which could not be adequately described by the use which the expression receives. The correct appraisal of the situation, however, is that the intuitive model is not anything that is definite. This leaves us with the consequence that there is in fact no definite meaning to the expression "natural number". We will assume, with Dummett, that the meaning of natural number involves the criterion for asserting something about all natural numbers in addition to the criterion for recognizing whether something is a natural number.[22]

Dummett has correctly made the point that what Gödel's theorem does in fact show is that "natural number" does not have a completely definite meaning.

> The only way to explain the meanings of quantification over the natural numbers is to state the principles for recognising as true a statement which involves it; Gödel's discovery amounted to the demonstration that the class of these principles cannot be specified exactly once for all, but must be acknowledged to be an indefinitely extensible class.[23]

This is to say that for any precise and definite account of the meaning of "natural number" there are aspects of the meaning of "natural number" which are not in the given account.

Wittgenstein, in the Philosophical Investigations, understood that there was no absolute criterion which governed the meaning or use of the term "number". His opinion on the matter was that numbers and games were subject to the same type of grouping; they formed a family and, as such, did not have any well-defined property that identified the group at large, rather, there are only family resemblances that hold between the members of the group.

> Why do we call something a "number"? Well, perhaps because it has a - direct - relationship with several things that have hitherto been called number; and this can be said to give it an indirect relationship

> to other things we call the same name. And we extend our concept of number as in spinning a thread we twist fibre on fibre. And the strength of the thread does not reside in the fact that some one fibre runs through its whole length, but in the overlapping of many fibres.[24]

A limit is placed on what any reasonable theory of meaning can achieve. We should not expect a rigorous, informative explanation which completely describes the meaning of "natural number". This does not mean that there is a definite meaning to the expression "natural number" which we are unable to describe adequately. On the contrary, we are unable to provide a rigorous and exhaustive description of its meaning because of the fact that it does not have a definite and clear-cut meaning.

FOOTNOTES TO CHAPTER FIVE

[1]Alfred Tarski, "The Concept Of Truth In Formalized Languages," p.159.

[2]A.N. Whitehead and Bertrand Russell, Principia Mathematica, I,92.

[3]We have used this wording so that primitive propositions such as *1.1 and *1.11 which are rules of Principia Mathematica are not included as proofs.

[4]Alan Anderson, "Mathematics And The 'Language Game,'" p.486.

[5]loc.cit.

[6]Kurt Gödel, "'Some metamathematical results on completeness and consistency,' 'On formally undecidable propositions of Principia Mathematica and related systems' I, and 'On completeness and consistency,'" p.606.

[7]ibid., pp.607-609.

[8]ibid., p.612.

[9]ibid., p.616.

[10]ibid., p.599.

[11]Georg Kreisel, "Wittgenstein's Remarks On The Foundations Of Mathematics," p.154.

[12]Anderson, op.cit., p.486.

[13]loc.cit.

[14]This issue was covered in Chapter Four.

[15]Kreisel, op.cit., p.153f.

[16]Michael Dummett, "The Philosophical Significance of Gödel's Theorem," p.186.

[17]*ibid.*, p.186f.

[18]*ibid.*, p.190.

[19]*ibid.*, p.189f.

[20]This is not a fault since a word such as "funny" might not have a precise and complete meaning.

[21]Dummett, *op.cit.*, p.190f.

[22]*ibid.*, p.194.

[23]*ibid.*, p.199.

[24]Ludwig Wittgenstein, *Philosophical Investigations*, § 67.

CHAPTER SIX

INTUITIONISM AND INVENTION

In this chapter I would like to examine Wittgenstein's remarks as they relate to the basic tenets of Intuitionism. Our attention will be occupied, for the most part, by investigating Wittgenstein's thoughts on the law of the excluded middle and the roles of discovery and invention in mathematical work.

Wittgenstein did not believe that one could be assured that a certain pattern of digits must either occur or not occur in the expansion of π if the pattern has not yet appeared. He looks at this problem from the perspective of men who are trained to provide an expansion of π by means of a certain rule. Wittgenstein believes that it is not at all clear that a pattern ϕ must either occur or not occur in the expansion of π given the fact that all they have to work with is the rule for the expansion. Something extra is required to support the assertion that the pattern ϕ either occurs or does not occur. Wittgenstein felt that such an assertion is given as a rule or postulate (<u>Remarks</u> p.138,no.9). The whole issue is an open question for Wittgenstein:

> What if someone were to reply to a question: 'So far there is no such thing as an answer to this question'?
> So, e.g., the poet might reply when asked whether the hero of his poem has a sister or not - when, that is, he has not yet decided anything about it.
> The question - I want to say - changes its status, when it becomes decidable. For a connection is made then, which formerly was not there (<u>Remarks</u> p.138,no.9).

It is apparent that Wittgenstein does not hold the view that every mathematical question is decidable as this is made abundantly clear in the analogy which he draws between the occurrence of a pattern of digits in the expansion of π and the existence of a poetic hero's sister. The rule for the expansion of π corresponds to the poem to the extent it has been completed and the poet's plans for its completion. If we cannot find sufficient information in the poem to ascertain whether the hero has a sister or not, we have no other recourse than to ask the poet. Now, if the poet has not given any thought to this issue, he may correctly answer our query with Wittgenstein's reply that there is no answer to the question. There is no other place where an answer to the question may reside.

The fact that we cannot discover whether the hero of the poem has a sister or not does not depend on the limitations which are imposed on our knowledge. This case differs from the question about an actual person's sister. We may not be able to come up with any answer when the question is asked in regard to a baby who, for example, was the sole survivor of a disaster in some extremely remote village. Here we should have no doubt that the child must have either had a sister or not had a sister. An answer to the question does indeed exist but an absence of information prevents us from finding it.

It is somewhat comforting to know that God has the same problem as we experience in determining whether a particular pattern occurs in the expansion of π. He deals with this problem by calculating according to the rule. If he does not calculate, the rule by itself will not give him the answer. The chapter and verse are:

> Suppose that people go on and on calculating the expansion of π. So God, who knows everything, knows whether they will have reached '777' by the end of the world. But can his omniscience decide whether they would have reached it after the end of the world? It cannot, I want to say: Even God can determine something mathematical only by mathematics. Even for him the mere rule of expansion cannot decide anything that it does not decide for us.
>
> We might put it like this: if the rule for the expansion has been given us, a calculation can tell us that there is a '2' at the fifth place. Could God have known this, without the calculation, purely from the rule of expansion? I want to say: No (Remarks p.185,no.34).

This quotation highlights Wittgenstein's uncompromising attitude towards a mathematical rule of expansion: One must execute the expansion (calculate) if one is to obtain any information from the rule. This leaves open two possibilities. The question about the appearance of a sequence of digits may be decided, in principle, by expanding the number or there might not be any answer to the question at all. These two possibilities correspond respectively to questions about finite and infinite expansions.

Fogelin makes the point that Wittgenstein ascribes the expectation of an answer to any question about an expansion to a confusion of the finite with the infinite.[1]

He refers to (Remarks p.139f,no.11) where Wittgenstein indicates that all questions involving the appearance of a particular sequence of digits in any finite expansion may be answered at least in principle. The finite expansion of a rule completely answers our questions of that finite segment:

> For an expansion of finite length, we can simply generate it and see if the pattern occurs. If it does occur, then it is obviously prescribed by the expansion rule; if it does not occur, then it is proscribed, in virtue of the fact that the rule of expansion permits only the one sequence that it in fact generates.[2]

Wittgenstein maintained the position that a rule must either prescribe or proscribe an answer to every question involving a finite expansion, however, he does not attribute this property indiscriminately to every rule when infinite expansions are considered.

> The opposite of "it must not occur" is "it can occur". For a finite segment of the series, however, the opposite of "it must not occur in it" seems to be: "it must occur in it" (<u>Remarks</u> p.143,no.18).

Fogelin uncovered the crucial factor in Wittgenstein's technique. Wittgenstein prefaces mathematical assumptions, employed in indirect proofs on the basis of the law of the excluded middle, with an operator that declares that the statement following the operator is the result of a rule. This leads Wittgenstein to interpret "pv $\sim$ p" as "RpvR $\sim$ p". "Rp" is read "there is a rule that p".

Thus, if we should derive a contradiction from the negation of a statement, and then assert its affirmation, we are actually performing our reasoning according to the following pattern:

$$\begin{array}{c} R \sim p \\ \vdots \\ \underline{q \,\&\, \sim q} \\ Rp \end{array}$$

The proper pattern should be:

$$\begin{array}{c} \sim Rp \\ \vdots \\ \underline{q \,\&\, \sim q} \\ Rp \end{array}$$

When we accept this first pattern we are tacitly giving our allegiance to the conditional statement $\sim Rp \rightarrow R \sim p$.[3]

This is tantamount to the assumption that there is an answer to every mathematical question. Gödel had a very deep belief that this assumption was true with respect to the continuum hypothesis as he attributed the undecidability of this question to problems in areas other than mathematics.[4] Wittgenstein, as we have seen, does not hold this position for all questions as he has committed himself to the decidability of questions of finite character. Where Gödel presupposes the existence of a mathematical domain which can, in principle, determine the answer to most questions, Wittgenstein rejects any such domain which will answer our questions about the appearance of a pattern of digits in the expansion of π.

Fogelin demonstrates that the remarks of Wittgenstein on the excluded middle are not in opposition to his avowal that he does not intend to alter mathematical methods. Wittgenstein is not against the use of the excluded middle in mathematics. His intention is to clarify the fact that we are creating, ex nihilo, new entities in the empty void when we use the law of the excluded middle in an indirect proof as evidence for the existence of an answer:

> Through the use of an indirect proof, the mathematician does establish something about a system: he shows that the introduction of "Rp" will lead to inconsistency. If in virtue of this he goes on to treat "R-p" (or "-P") as a theorem, he has, in effect, laid down a new rule or made a new decision. Looked at in this way, the method of indirect proof becomes an engine of creation and not, as the Platonists believe, a device for discovering new mathematical facts. Alternatively, we can say that the classical mathematician accepts a second order rule to the effect that anything is a rule if the introduction of the contrary rule leads to inconsistency. We can call this the principle of mathematical growth.[5]

This understanding of Wittgenstein's views on the excluded middle is very similar to the analysis, in Chapter Two, of Wittgenstein's remarks on infinity. In both instances Wittgenstein does not reject the mathematician's use of the techniques but establishes the fact that these techniques are the results of great leaps from the techniques which were previously employed. Wittgenstein, in describing the appearance of the pattern ϕ, states, "Only within a mathematical structure which has yet to be erected does the question allow of a mathematical decision, and at the same time become a demand for such a decision (Remarks p.144,no.20). We should, however, exercise caution that we do not confuse a non-constructive existence proof based on the law of the excluded middle with a constructive proof. In the case of a non-constructive proof we do not know where the pattern will occur whereas in a constructive proof we can see where the pattern occurs or at least possess a procedure which will, after a finite number of steps, yield the desired information about the location.

We may now compare Wittgenstein's mathematical beliefs with those of the intuitionists. First of all, the similarities between Wittgenstein and the intuitionists will be noted. Our comparison will deal with the very strict form of intuitionism as espoused by Brouwer.

Wittgenstein and Brouwer are in substantial agreement with respect to the applicability of the law of the excluded middle. Brouwer has no reservations concerning finite instances of this law.[6] He was opposed to the use of this principle in infinite cases and, in fact, ascribed much of the assurance which the law of the excluded middle enjoys to our experience with it in those cases where a single assertion (an assertion about one object) is made and the validity of the law in everyday life.[7] The faithfulness of this law in these finite circumstances induces us to trust it completely in every situation. This matches very well with Wittgenstein's comments on the decidability of questions dealing with the appearance of a pattern in any finite segment of the expansion of π.

Intuitionistic logic differs from classical logic as a result of dissimilar approaches to the question of truth and provability. The intuitionist does not admit the truth of a statement that is not provable. For this reason the intuitionist accepts "$A \vee \sim A$" as true in exactly those cases where either A is proved or $\sim A$ is proved.[8] This motivation prompted Wittgenstein to interpret "$A \vee \sim A$" as "$RA \vee R \sim A$" where "R" is Fogelin's notation for "it is a rule that".

These similarities are quite striking inasmuch as most of intuitionistic mathematics receives its structure from these basic principles. The respects in which Wittgenstein differs from the intuitionists, however, are equally as striking. One will notice that the intuitionists' claims are intended to change the actual practice of mathematics while Wittgenstein does not carry with him such an antagonistic programme towards mathematical practice. The intuitionists, for example, will have absolutely nothing to do with the classical mathematician's use of the law of the excluded middle in relation to infinite sets.[9] Wittgenstein agreed with the reasoning that the intuitionists use in their counter-examples to the classical law of the excluded middle but he concluded that these efforts demonstrate that a new area of mathematics, not a bad mathematics, has been invented.

Though Wittgenstein and the intuitionists coincide in their belief that mathematics is a subject of invention rather than discovery, the intuitionistic doctrine on the nature of this invention is completely at odds with Wittgenstein's basic proposals. The intuitionistic doctrine, in its purest form, is characterized by the fact that it considers mathematics to be a mental activity. Brouwer is very stringent in keeping out any foreign elements. This is seen in his statement that mathematics is

> ...an autonomic interior constructional mental activity which although it has found extremely useful linguistic expression and can be applied to an exterior world, nevertheless neither in its origin nor in the essence of its method has anything to do with language or an exterior world,...[10]

This interior mental activity is based on the belief that time is a priori. Mathematics is built up as follows:

> This neo-intuitionism considers the falling apart of moments of life into qualitatively different parts, to be reunited only while remaining separated by time, as the fundamental phenomenon of the human intellect, passing by abstracting from its emotional content into the fundamental phenomenon of mathematical thinking, the intuition of the bare two-oneness.[11]

These last two quotations establish the extreme nature of the intuitionistic philosophy of mathematics. Brouwer's beliefs regarding time have exceeded the limitations which Kant perceived. From the fact that mathematics, and hence the intuition upon which it is based, is in its essence, independent of the external world, we may discover that Brouwer maintains a view of time which Kant attacks in his "Refutation of Idealism." Our apprehension of time depends upon the existence of the external world.[12] We will not elaborate further on this point but will rest content in making known Brouwer's departure from Kant.

Brouwer's philosophy of mathematics roots itself in ground that is poison for Wittgenstein. We have seen in Chapter One that following a rule is a practice. In the case of Brouwer's "fundamental phenomenon of the human intellect" and the rules which are intended to connect it with mathematical statements, there appears to be nothing definite which can actually be described as an instance of following the rule.

Normally we can check on a person's calculation and determine whether he has correctly followed the rules of arithmetic. If an intuitionist were to determine that 2+2=3 on the basis of a Brouwerian calculation which is independent of both language and the external world, there would be no way of deciding whether this intuitionist correctly translated his intuition into mathematical language. The following rhetorical question seems appropriate: "Are you sure - one would like to ask - that this is the correct translation of your wordless thought into words?"[13] As in the case of the private linguist, whatever the intuitionist thinks to be an instance of following the rule will be an instance of following the rule. Thus, it is not proper to say that a rule is in fact being followed.[14]

This problem stems from Brouwer's unwarranted emphasis upon a mental act, independent of language and the external world, which supports every mathematical construction. This reveals the fundamental difference between Brouwer and Wittgenstein. Whereas Brouwer advocates that a rule is essentially a private mental process, Wittgenstein proposes the position that a rule belongs in a public framework.

FOOTNOTES TO CHAPTER SIX

[1]Robert Fogelin, "Wittgenstein And Intuitionism," p.269.

[2]loc.cit.

[3]loc.cit.

[4]Kurt Gödel, "What Is Cantor's Continuum Problem?" p.263.

[5]Fogelin, op.cit., p.270.

[6]L.E.J. Brouwer, "Consciousness, Philosophy, And Mathematics," p.79.

[7]ibid., p.82.

[8]Michael Dummett, Elements Of Intuitionism, p.18.

[9]L.E.J. Brouwer, "On the significance of the principle of excluded middle in mathematics, especially in function theory," p.336.

[10]L.E.J. Brouwer, "The Effect Of Intuitionism On Classical Algebra Of Logic," p.551.

[11]L.E.J. Brouwer, "Intuitionism And Formalism," p.69.

[12]Immanuel Kant, Immanuel Kant's Critique Of Pure Reason, p.245.

[13]Ludwig Wittgenstein, Philosophical Investigations, §342.

[14]Since intuitionism provides a method or rule for constructing mathematics, it is appropriate to consider which circumstances would constitute an instance of following that rule.

CHAPTER SEVEN

MATHEMATICS AND LANGUAGE-GAMES

In this chapter I will attempt to show that mathematics is a family of language-games. This is a combination of two Wittgensteinian themes - family resemblances and language-games. In section (A) the notion of language-games and its connection with mathematics will be briefly examined. Section (B) devotes itself to demonstrating that there is not necessarily one family trait which runs throughout mathematics and sets it apart from other disciplines. This will involve a comparison of arithmetic and large cardinal axioms. Section (C) examines the resemblances which link the various mathematical disciplines together.

(A) Language-Games

Wittgenstein introduced the concept of language-games in the early sections of Philosophical Investigations to show that the learning and use of a language are activities. For example, the process by which a pupil might learn words such as "block", "pillar", "slab", and "beam" are language-games.[1] Language-games can become increasingly complicated. The key elements in a language-game are the presence of a language and the actions into which the language is woven.[2]

The following activities are further examples, given by Wittgenstein, of language-games:

> Giving orders, and obeying them -
> Describing the appearance of an object,
> or giving its measurements -
> Constructing an object from a description

(a drawing) -
Reporting an event -
Speculating about an event -
Forming and testing a hypothesis -
Presenting the results of an experiment
in tables and diagrams -
Making up a story; and reading it -
Play-acting -
Singing catches -
Guessing riddles -
Making a joke; telling it -
Solving a problem in practical arithmetic -
Translating from one language into another -
Asking, thanking, cursing, greeting, praying.[3]

In Chapter One we have seen that the following of a mathematical rule is a practice. This activity of following a mathematical rule constitutes a language-game. The language of mathematical rules is woven into the practice or activity of following a rule (Remarks p.186,no.35). Thus in saying that mathematics consists of a family of language-games, we are stressing the fact that following a mathematical rule is a practice.

(B) Large Cardinals[4]

Set theory is an area of mathematics which provides a rigorous account of our intuitive notion of collections. This intuitive notion is also augmented so that we can speak of sets such as the set of natural numbers and the set of real numbers. The most popular account of set theory consists of the Zermelo-Fraenkel axioms with the axiom of choice (ZFC). We may briefly note that sets in ZFC can be viewed as a hierarchy of sets built upon two basic sets. These two basic sets are the empty set which has no members and an infinite set which is essentially the set of natural numbers. Every set whose existence is guaranteed by ZFC is the result of a reiterated application of the axioms of ZFC to either of these two basic sets.

Set theory is quite powerful in that standard areas of mathematics such as geometry, analysis, algebra and topology can be reconstructed within ZFC. That is to say one can interpret lines, real numbers, groups, etc. as sets and provide in ZFC the proofs which correspond to those of standard mathematics.

The consistency of ZFC is assumed rather than proved. This is due to Godel's second incompleteness theorem which states that given any consistent formal system sufficiently strong to yield arithmetic, the consistency of that system cannot be proved within the formal system.

The hierarchy of sets forms a universe V which is defined as follows.

$$V_0 = \emptyset$$

$$V_{\alpha+1} = \mathcal{P}(V_\alpha) \quad \text{where } \mathcal{P} \text{ is the power set operation}$$

$$V_\alpha = \bigcup_{\beta<\alpha} V_\beta \quad \text{when } \alpha \text{ is a limit ordinal}$$

Thus the universe of sets is indexed by ordinals which indicate the stage at which each level of the hierarchy is formed. Whenever the existence of the indexing ordinal α is guaranteed by ZFC, the corresponding level of the hierarchy V_α is also guaranteed by ZFC.

If there are levels in the hierarchy of sets which are not guaranteed by ZFC, then there must be ordinals, indexing these levels, whose existence is not guaranteed by ZFC. Such an ordinal could not be reached by the reiterated application of the axioms of ZFC. The study of large cardinals is an area of set theory in which this notion of a set, not reached by the reiterated application of the ZFC axioms to a set that is guaranteed by ZFC, is expanded. With large cardinals we are brought into one of the most abstract areas of modern mathematics.

An inaccessible cardinal is a cardinal which cannot be reached by the reiterated application of the ZFC axioms to any set which is on a lower level in the hierarchy of sets.[5] The sets which are on a lower level do not have to be guaranteed by ZFC. Inaccessible cardinals bear upon the consistency of ZFC; furthermore one cannot prove in ZFC the existence of inaccessible cardinals.

If there are inaccessible cardinals and we take κ as the least such, then V_κ is a model of ZFC+ "there is no inaccessible cardinal." Hence the existence of inaccessible cardinals implies the consistency of ZFC. On the other hand, if there are no inaccessible cardinals in the set theoretic universe, we may take the universe as a model of ZFC+ "there is no inaccessible cardinal." In either case we have a model of ZFC+ "there is no inaccessible cardinal," hence it cannot be proven in ZFC that there are inaccessible cardinals. We also make note of the fact that the relative consistency of ZFC and "there is an inaccessible cardinal" has not been shown.

The introduction to the general notion of an inaccessible cardinal facilitates an indication of the size of some large cardinals. The interested reader can obtain the necessary definitions and proofs from the work mentioned in footnote four. There are a number of indications of the size of large cardinals. I have chosen the simplest of these indications.

The first of the very large cardinals which we will consider are the measurable cardinals. If κ is a measurable cardinal, then κ is the κ th inaccessible cardinal. Thus, it is an extremely large cardinal.

A supercompact cardinal is larger than one that is simply measurable. In fact, if κ is a supercompact cardinal, κ is the κ th measurable cardinal.

Extendible cardinals are another group of large cardinals. The reader will not be surprised to hear that if κ is an extendible cardinal, then κ is the κ th supercompact cardinal.

Our next leap into the beyond takes place via Vopenka's principle. The truth of this principle would imply that the collection of extendible cardinals forms a proper class.

The last of the large cardinals which we will mention are the huge cardinals. If a huge cardinal exists then Vopenka's principle is consistent. Huge cardinals are about as big a cardinal as one could ever want for, as

Kunen proved, "With the definition of huge cardinals, we are approaching the brink of inconsistency."[6]

I believe that the situation with large cardinals contrasts sharply with what we find in elementary arithmetic. Whereas "2+2=4" is firmly entrenched in our concept of rationality, a statement such as "there are extendible cardinals" is accepted by a very small community consisting of a subset of mathematicians who are familiar with the theory of large cardinals. The attitude which one maintains towards the existence of certain large cardinals does not reflect on one's ability to reason. In the first chapter we examined the necessity that accompanied a statement such as "2+1=3". The factors which come into play in accepting a proposition about the existence of various large cardinals are factors which are not considered as essential. The acceptance of large cardinal propositions depends upon one's desire for practicality, a philosophy of how the set theoretic universe should behave, the extent to which one is liberal or conservative, one's taste in aesthetic matters, etc.

The whole theory of large cardinals certainly constitutes a new subject area within set theory. It is a new language-game. I have emphasized the fact that a number of large cardinals α can be described as the αth cardinal of some other large cardinal category. This hierarchy taxes one's imagination to the point that an adequate understanding of the material is not achieved. The introduction of various large cardinals into our set theoretic apparatus should be viewed in the same light as the introduction of the axiom of infinity and such statements as "$2^{\aleph_0} > \aleph_0$". We do not have prefabricated notions at our disposal which can capture the content of large cardinal theorems. Again it is necessary to look upon the techniques which are used to support these apparently strange statements. The mathematical analysis of these concepts places our feet upon the ground as it deals with them in terms of formulas that involve such notions as ultrafilters.[7] What we have here, in fact, is a new language-game with a set of new mathematical rules.

Mathematical language-games need not share a common characteristic that clearly identifies them. This is most aptly illustrated by setting elementary mathematics and large cardinals side by side for comparison. I would like to submit that they are at opposite ends of the mathematical spectrum. The most important difference is given in the following paragraph.

The certainty of elementary mathematics is not questioned and it is applied without trepidation to many areas. Geography, economics, sports, art criticism and sociology are among the fields that are open to arithmetical methods. Large cardinals are employed in few other areas and the possibility of a contradiction arising from the introduction of a large cardinal axiom to a set theory such as Zermelo-Fraenkel cannot be dispelled within the set theory in question.

The natures of these two subjects are disparate to the point where one might be tempted to say that all they have in common is the fact that they are taught under the name of "mathematics" in our educational system. There are a number of common characteristics between such subjects as algebra and set theory; however, there is no trait that uniquely pervades all of mathematics. Although the description of mathematics as a collection of language-games does not provide us with a rigorous, crystal clear account of mathematics that will enable a person to ascertain whether any particular piece of work is mathematical,[8] this is a virtue (we are making a virtue out of necessity) since mathematics is sufficiently varied that we cannot hope for more than this. This leaves us with a number of mathematical theories. The interesting material is found in particular aspects of these subjects.

Thus, we obtain informative answers when we ask questions dealing with infinity in set theory, Dedekind's theorem, Gödel's incompleteness theorem, arithmetic and reasoning, and formal systems. General theories on mathematics, if they are to capture all of mathematics, will be watered down to the point where very little accurate information can be conveyed. This explains the disjoint nature of Wittgenstein's remarks. His philosophy of mathematics thrives on examples. The problems which interested Wittgenstein in regard to Dedekind's theorem might have very little relevance towards other mathematical areas. Problems of this sort are helpful in that they bring to light aspects of mathematics that are suppressed when philosophy assays to impose unity upon a recalcitrant mathematics.

(C) Mathematical Resemblance

We have stated that mathematics consists of a collection of language-games and that there is not necessarily one common thread that runs throughout mathematics that sets it apart from other disciplines. This does not mean that mathematics consists of a totally arbitrary collection of subject areas. We may compare the situation with games. There is no significant property that belongs to all games and identifies them as such. There are, however, properties that apply to a large number of games and overlap with other properties which also apply to a number of games. Professional football and boxing are examples of games which are performed, respectively, with and without a ball. On the other hand, they are both contact sports performed in front of a paying audience. The fact that both are games is not, as a result, totally fortuitous.

I wish to illustrate that "mathematics", like "games", is not a capricious concept. Wittgenstein made this point: "Mathematics is, then, a family; but that is not to say that we shall not mind what is incorporated into it" (Remarks p.180,no.26). In particular, to paint the complete picture, it is incumbent upon me to establish that the subject of large cardinals does have some connection with less abstract areas of mathematics.

We argued in Chapter Two and Three that mathematics must invent or create infinite sets such as the set of real numbers. We have also argued in the present chapter that large cardinals are also inventions. They are creations in that we now have a set whose existence was not previously guaranteed. Nevertheless, a large cardinal does not live in a world of its own. They extend a universe of sets; they do not create a totally new universe. A new axiom is added to those of Zermelo-Fraenkel but the set corresponding to the axiom is treated in exactly the same manner as any set which exists in the universe of Zermelo-Fraenkel. The point which we are making amounts to nothing more than the following: any theorem of Zermelo-Fraenkel is a theorem of Zermelo-Fraenkel plus a large cardinal axiom.

The proofs of set theory do share with the proofs of elementary mathematics the property of necessity in that the statement which is proved necessarily follows from the premises of the argument. This explains the fact that a mathematician who, for whatever reason, is radically opposed to a large cardinal axiom may still agree with the fact that the axiom does in fact lead to the theorems proved by another mathematician who accepts the large cardinal axiom. The important difference is found in the mathematician's attitude towards the nature of an acceptable premise. In the case of elementary mathematics, "2+2=4" can be deduced from the equation "2+1=3". The latter equation is not to be doubted.[9] From the theory of large cardinals let us take the following two statements: "a supercompact cardinal exists" and "there is a measurable cardinal". The second statement may be deduced from the first. Both deductions consist basically of substitutions of identity.

The important difference between the two deductions lies in the two premises. Whereas "2+1=3" is not in the least controversial, the existence of a supercompact cardinal is up for grabs as it were. Although the considerations which are involved in accepting a large cardinal axiom are not applicable to the acceptance of "2+1=3", there is a relationship between these two areas of mathematics. The relation is both historical and logical. The historical roots of set theory are grounded in Cantor's work on the convergence of a particular trigonometrical series.[10] The roots of this subject in turn sprang from arithmetic and geometry. This historical connection is not completely accidental for a similar relation exists in the logical nature of set theory. For example, the development of the finite cardinals in set theory corresponds to the whole numbers of arithmetic. The set theoretician does not want "2+1=4" as a theorem of his subject. This faithfulness towards arithmetic provides a humorous instance of a very large cardinal axiom, namely, "0=1".[11] It is comforting to know that this equation is also a contradiction in set theory. A more serious example of the logical connection between set theory and analysis is established in a theorem by Solovay which states that the existence of an inaccessible cardinal implies that there is a model of ZF+DC (Zermelo-Fraenkel set theory with the Axiom of Choice replaced by the principle of dependent choices) wherein every set of real numbers is Lebesgue measurable.[12]

Thus, the realm of set theory has certain ties with the more concrete regions of mathematics. We are not saying that there is one thread which unites all of mathematics. Our point is that there are links between one mathematical area and another. I believe that these observations enable us to answer the following question which Wittgenstein asked of mathematical areas such as set theory, "...don't we call it 'mathematics' only because e.g. there are transitions, bridges from the fanciful to non-fanciful applications?" (Remarks p.180,no.25). The answer is "Yes". Without the bridges and transitions, the large cardinal axioms would be very much like an interesting fairy tale about some enormous creature who was, due to his unusual behaviour, misunderstood by everyone who met him. The large cardinal axiom is fanciful in that it proposes the existence of an object which does not seem to have any connection with reality. Upon examination, however, it is seen that our large cardinal cannot be dealt with in just any arbitrary fashion, motivated solely by one's imagination and desire to entertain. It is subject to mathematical methods that are every bit as rigorous as the methods involved in basic arithmetic. Furthermore, we have seen that the entities and methods of set theory can be traced back ultimately to elementary arithmetic. The mere fact that a large cardinal axiom does not carry the necessity and immediate application of "2+1=3" does not entail that large cardinals should be excluded from mathematics. The line of descent warrants the legitimacy of large cardinals' inclusion in mathematics.

This characterization of mathematics is sufficiently strong to prevent the game of chess, for example, from gaining admission into the area of mathematics. First of all, there are significant differences between it and arithmetic. The operations of arithmetic have their immediate application with counting which convinces us that 2+1 must equal 3. There is no such application in the case of chess. The connection between the chessman and certain ancient methods of war is accidental to the playing of the game. There is no question such as "why are the rules of chess true". The important matter is to master the rules of the game whereas it does make sense to ask why arithmetical rules are true.

The second point which must be shown is that chess does not belong to what I have called "the line of descent". Here we can see that there is some similarity between chess and an abstract area of mathematics in that neither of them has an immediate application which forces us to accept their truth. There is however, a major difference that separates them with respect to mathematics.

Whereas an abstract area such as set theory has generalizations of arithmetic and is in a sense an elaboration of arithmetic, chess is independent of any area of mathematics. This is to say that there is no mathematical operation which must be performed when one makes a move in chess nor are these operations a development of any area of mathematics. There might be circumstances in chess where it is helpful to base a move on some mathematical information but this does not mean that making that particular move is doing mathematics. The most we can say is that we can in certain cases apply mathematics to a game of chess in much the same way as one can apply probability theory to a game of poker without turning poker players into mathematicians.

FOOTNOTES TO CHAPTER SEVEN

[1]Ludwig Wittgenstein, Philosophical Investigations, §§2,7.

[2]ibid., §7.

[3]ibid., §23.

[4]The mathematical material pertaining to sets and large cardinals in this section is found in Thomas Jech, Set Theory.

[5]An inaccessible cardinal is properly defined as an uncountable, regular, strong limit cardinal.

A cardinal κ is regular if there is no cardinal $\beta < \kappa$ such that $\kappa = \bigcup_{\alpha<\beta} X_\alpha$ where each X_α is a set whose cardinality is smaller than κ.

κ is a strong limit cardinal if $2^\alpha < \kappa$ for all $\alpha < \kappa$.

[6]Thomas Jech, Set Theory, p.416.

[7]The development of ultrafilters and their use in the theory of large cardinals is beyond the scope of this dissertation. The important point is that certain ultrafilters provide mathematical methods for comparing the size of large cardinals.

[8]There are two descriptions which I am excluding. The first description, that mathematics consists of number theory, analysis, algebra, etc., is certainly true but trivial. The second characterization is that mathematics is basically set theory. Wittgenstein's account of Principia Mathematica examined in Chapter Four is applicable to this issue. For the same reasons that Russellian arithmetic is not the same as ordinary arithmetic, the essence of mathematics is not set theory.

[9]The connection between addition and counting which is stated in Chapter One is presupposed here.

[10]Georg Cantor, Contributions To the Founding Of The Theory of Transfinite Numbers, p.24ff.

[11]A. Kanamori and M. Magidor, "The Evolution Of Large Cardinal Axioms In Set Theory," p.265.

[12]Thomas Jech, op.cit., p.537.

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